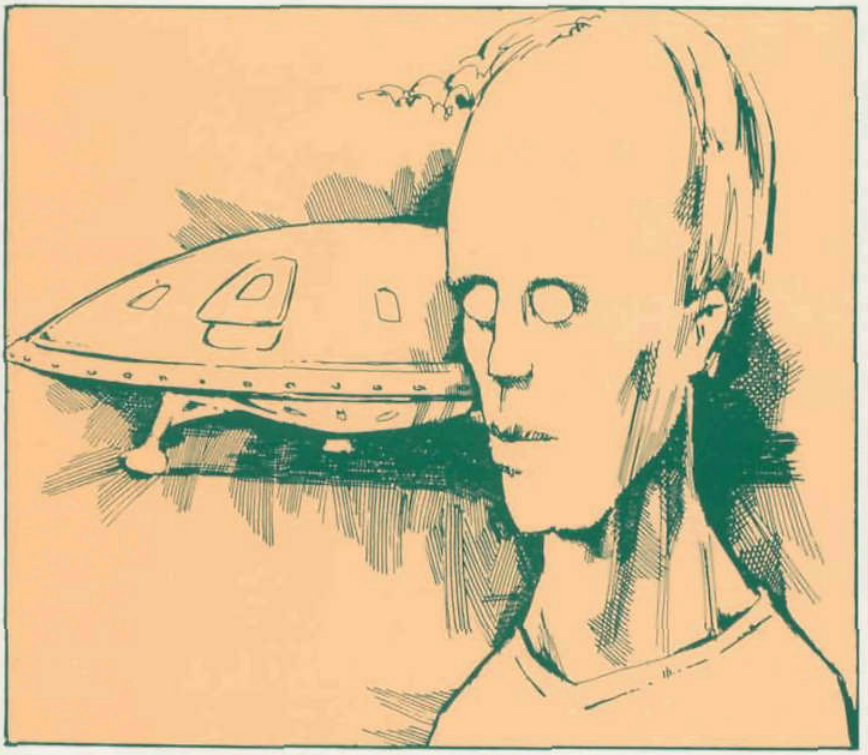
the Skeptical Inquirer

Hypnosis and UFOs



Schmidt's PK Experiments
Deciphering Ancient America
A Sense of the Ridiculous

Published by the Committee for the Scientific Investigation of Claims of the Paranormal
VOL. V NO. 3 SPRING 1981

the Skeptical Inquirer
THE ZETETIC

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Cover illustration by Tom Toles

From the Chairman

It has been five years since the Committee for the Scientific Investigation of Claims of the Paranormal was founded. This is an occasion to mark our anniversary and to contemplate the future.

When we held the first meeting of the Committee at the State University of New York at Buffalo, on May 1, 1976, under the sponsorship of the American Humanist Association, little did we know that we would grow so rapidly. We are now an independent nonprofit scientific organization; we have the cooperation of more than 200 investigators in the United States and eight other countries; and we have established active UFO, Education, and Paranormal Health Claims subcommittees. We did not imagine then that our efforts would strike such a responsive chord among the scientific community, the media, and the general public. Nor did we anticipate that the SKEPTICAL INQUIRER, under the competent editorship of Ken Frazier, would attract such a wide and loyal audience—we now have almost 7,500 subscribers. Granted we cannot compete with the mass-market magazines; yet our circulation continues to grow and our influence far outweighs our numbers. Articles that appear in the SKEPTICAL INQUIRER serve as a valuable resource—virtually the only skeptical one available—and are constantly quoted in magazines, newspapers, and by radio and TV commentators throughout the world; many science magazines, such as *Science, Scientific American, New Scientist, Discover, Omni,* and the French *Science et Vie,* report our activities and draw heavily on our findings.

The reasons for forming the Committee are familiar to those who have been with us from the start. There has been an enormous increase in belief in the paranormal, and until we appeared on the scene it went largely unchallenged. The paranormal viewpoint had a virtual monopoly in the mass media. We thought that there ought to be an opportunity for the public to hear about scientific approaches to such claims. We also thought it would be useful for scientists to take the responsibility for examining paranormal claims and to publish their findings. It also seemed that it would be helpful to organize, however loosely, a network of skeptical inquirers in order to coordinate investigative efforts. However, we have insisted from the beginning that, though we may be skeptical, we cannot prejudge paranormal issues but must submit them to objective examination, careful research, and fair-minded analysis.

CSICOP has developed over the past five years with surprisingly little strife, given the strong feelings pro and con that the paranormal engenders. The Commit-

tee has not been without internal stresses, however, and these have been of two kinds. First, there are those who think we are "too academic" and too restrained in our criticisms and that we should be more aggressive in our efforts to refute the rising tide of nonsense. Second, there are those who maintain that we ought not to enter the public arena of discourse but simply publish a scholarly scientific journal, stating the facts as we see them and leaving it at that. We have attempted to strike a balance between these two conceptions. On the one hand, we have sought to provide critical information for the general public and the media—our educative function—and, on the other, we have attempted to contribute to scientific research, however modestly. Of course there have been criticisms, some quite intense, from disciples of the paranormal who have felt threatened by our existence. It is a historical fact (going back at least to Thebes) that the bearers of sad tidings are often condemned. Those who promise great things are preferred to those who, after looking at the evidence, doubt them. Magazines committed to the paranormal, such as *Fate,* have engaged in ad hominem attacks on members of CSICOP; and astrologers, "psychics," UFOlogists, and assorted other pseudoscientists consider the Committee to be their chief enemy. But we believe it is important to maintain a dialogue and to cooperate wherever possible with serious paranormal researchers. Indeed, Committee members have debated J. B. Rhine, Charles Tart, Helmut Schmidt, and others in parapsychology; J. Allen Hynek, Allan Hendry, and Bruce Maccabee in the UFO field; Michel and Francoise Gauquelin in astrobiology; and many others. We want to continue to do so.

My most serious concern is that, although five years ago interest in the paranormal was considered a fad—many did not take it seriously or saw little harm in it—today paranormal attitudes have deeply penetrated the public consciousness and are generally accepted as valid. Although CSICOP has undoubtedly helped to crystallize the opposing viewpoint, we are still part of a small minority (along with other scientists). Constantly bombarded by grandiose paranormal claims in the mass culture, people are easily taken in—even some scientists are prey to the lure. Professors in the universities and colleges report with dismay the prevalence of uncritical belief in the paranormal among their students—students in journalism, psychology, anthropology, astronomy, philosophy, and other fields.

The skeptic is like the Dutch boy at the dike attempting to put his fingers in all the new holes that keep appearing. Some so-called superpsychics who have been thoroughly refuted disappear from the public eye for a short time only to return and gain new adherents. Jeane Dixon, for example, is an unsinkable rubber duck; no matter how many times she is knocked down, she still pops up again with new predictions. The *National Enquirer,* though thoroughly discredited, has all too often been the model for the general media. The "Amityville Horror" story had been proved a hoax when it was published in hardcover, but it went on to become a best-seller when it came out in paperback, and the movie was a box-office hit. Astrology, though unfounded, is as strong as ever. Has ESP been demonstrated in the laboratory?—we can debate that issue as scholars. But if just one performer does a "psychokinetic" trick on a television show, millions of viewers are convinced of its authenticity. Recently, the producers of a British documentary debunking psychic surgery were astounded when most of the hundreds of calls received after the program were from viewers who wanted to know where they could go to have psychic surgery performed!

Perhaps human beings crave myths—and the more fanciful the tale, the

greater the number of adherents. For many, paranormal belief apparently satisfies a psychological hunger. We can present the evidence concerning the paranormal—or the lack of it—but there is no guarantee that everyone will accept it.

I think CSICOP has a continuing role to play, but it will be an up-hill battle. We have no large foundations, educational institutions, or governmental agencies to sustain us, and our survival literally depends upon the contributions of the readers of the SKEPTICAL INQUIRER and the fellows and scientific consultants of the Committee. We value suggestions and criticisms from you our readers, and we value your support. We are not infallible—no doubt we have made mistakes. We are a voluntary movement, inspired by a keen sense that we have a responsibility to do what we can to encourage the spirit of scientific inquiry and to cultivate the skeptical attitude and the quest for evidence and reason.

—*Paul Kurtz*

News and Comment

Notes on the 1980 National UFO Conference

The seventeenth annual National UFO Conference was held at the Doral Inn in New York City last summer. I was one of the invited speakers. The names of the participants and of those in attendance were a "who's who" of UFOlogy. The principal organizer and venture capitalist of the conference was James W. Moseley of Gutenberg, New Jersey, a long-time UFO enthusiast.

Day One: New York City freelance artist George A. Rackus set up his sculpted "close encounter" alien heads (see photo) in the room where the press conference was about to begin. Stanton T. Friedman, who calls himself the "Flying Saucer Physicist," strongly objected to this display, calling it "creative fiction." However, he approved of a similar sculpture of an alien head that alleged UFO-abductee Betty Hill carries wherever she goes and which was resting on the table next to him. I said that I could not discern any real difference between the "authentic" alien sculpture and the fictionalized ones.

The press conference was well attended, mostly by the local media, and was reported by two major New York City TV news programs. Mike Luckman read a statement on behalf of the conference organizers that criticized Jimmy Carter for supposedly failing to release all government UFO information. The various presidential candidates were called upon to take a public position on UFOs and to state whether or not they would end the alleged cover-up of UFO secrets. (At that time Ronald Reagan and Gerald Ford were directly across the street at a fund-raising affair in the Waldorf Astoria, but they took no notice of the challenge.) Luckman made much of the supposed UFO sighting by Jimmy Carter. When my turn came to speak, I pointed out that Jimmy Carter's "UFO" was almost certainly the planet Venus. Stanton Friedman agreed that Carter probably did not see a genuine UFO and that the case has little merit. Friedman disagreed that the object sighted was Venus, but he did not say exactly what he thought it was.

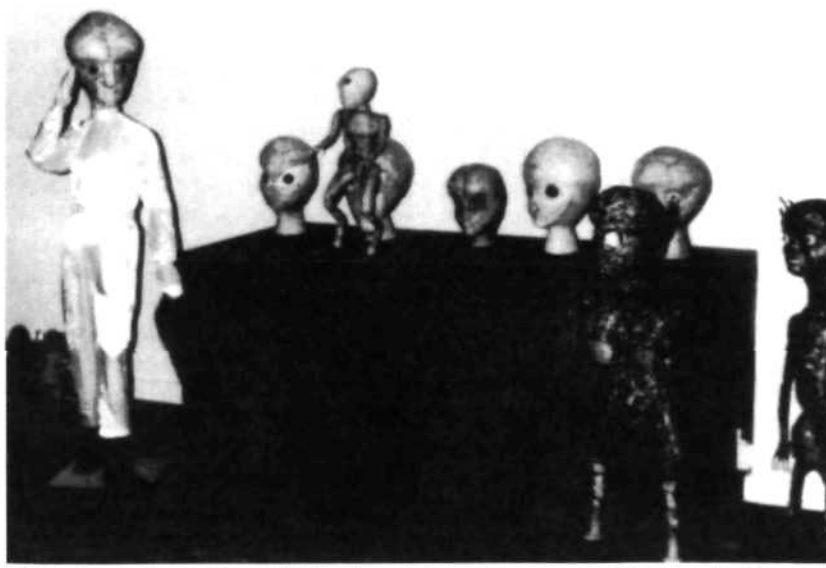
Friedman raised charges at the press conference of a "cosmic Watergate," alleging that the government has been suppressing evidence of "crashed saucers" for more than 30 years. The preserved bodies of little saucer creatures were said to be in the possession of the U.S. Government. He said that governments all over the world are hiding their saucer knowledge because none of them want their citizens developing "planetary consciousness."

Day Two: John Keel, author of *The Mothman Prophecies*, spoke on phantom submarines, mystery aircraft, and animal mutilations. One veteran observer of these conferences later said to me: "That sounded almost exactly like the lecture he gave here in 1967."

The next event was the debate between Stanton Friedman and me, the first public debate between a proponent and a skeptic at a major UFO conference. Friedman went first. He talked about the supposed mountain of eyewitness testimony about UFOs, without being specific. He cited the 2,000 alleged UFO landings catalogued by Ted Phillips of the Center for UFO Studies. He talked about abductions and radar sightings, again without specifics. He again talked about the supposed "cosmic Watergate," once more claiming a massive government cover-up. He talked about the Air Force's Blue Book *Special Report 14*, which broke down sightings into categories and then further into "knowns" and "unknowns" (or "unidentifieds"). Friedman cited the differences between the "knowns" and the "unknowns," claiming that these differences showed that "unknowns" were definitely something other than Venus or weather balloons.

In my response I pointed out that UFOs always manage to slip away somehow before the evidence becomes too convincing, even if they fly over major cities like New York or Los Angeles. The Great Rocky Mountain Meteor of August 1972, for example, appeared unexpectedly and was visible for less than a minute, yet its picture was taken by many independent photographers. No UFO has ever been so widely seen or photographed.

As for Blue Book's *Special Report 14*, I briefly summarized Allan Hendry's arguments against the categories it used. (Hendry is the chief investigator for the Center for UFO Studies and is the author of the well-regarded *UFO Handbook*, reviewed in the Winter



Robert Sheaffer

"Close Encounters of the Third Kind": sculptures of supposed UFO aliens by George A. Rackus.

1979-80 issue of this journal.) Hendry notes that, under the Blue Book categories, a city bus looks very much like an elephant: both are greyish, have four appendages, and move very slowly.

Regarding the alleged suppression of UFO information, I referred to Philip J. Klass's article in the Spring 1980 SKEPTICAL INQUIRER, which quoted CIA memoranda revealing a complete lack of serious interest in UFOs. Besides, I argued, penalties for disclosing official secrets are *not* as tough as Friedman asserts, as Daniel Ellsberg and Victor Marchetti discovered. Some CIA agent would surely have revealed the government's UFO secrets by now, I said, if there were any. I invited Friedman to become more specific about his mountain of UFO evidence: *which* cases are the indisputably reliable ones? He never responded.

I brought up the Delphos, Kansas, physical evidence case of 1971. A UFO was said to have landed on a farm in Delphos, leaving a mysteriously disturbed ring of soil behind. The principal witness, Ronald Johnson, said he had subsequently seen a "wolf girl" on the farm and that several of the animals had had virgin births.

Klass and others investigated the case, but no one was able to determine the cause of the ring. The *National Enquirer*'s Blue Ribbon UFO panel selected this case as providing the "most scientifically valuable" evidence of UFOs. But Jacques Vallee has recently had a sample of the soil analyzed in a laboratory in France, and he reports that the ring is actually a growth of the fungus Actinomy-cetacae, genus *Nocardia*. Friedman, who was not familiar with Vallee's finding, said that he could not accept that explanation.

After the debate, Moseley asked for an "applause meter" to see who had won. Not surprisingly, about three-quarters clapped for Stanton Friedman, one quarter for me. (Had the debate taken place after the next talk, I might have fared much better!)

Robert Sheaffer

Stanton Friedman selling UFO literature at the conference.

Our debate was followed by a slide lecture given by Betty Hill, the famous "UFO abductee" (1961) from New Hampshire. She showed well over one hundred slides of "UFOs" taken at the "UFO landing spot" where she goes three times a week. The UFOs have not only fired laser beams at her car, blistering the paint, but actually burned holes in the metal, she said.

One of Betty Hill's UFOs was jellyfish-shaped. She noted that a "jellyfish UFO" was seen over Petrozavodsk, USSR, in 1977 and had allegedly damaged windows. She did not seem aware that James Oberg has shown the sighting was of a satellite launching (*SI*, Fall/Winter 1977). Most of her photos are frames from movie film. What was obviously a bright meteor in one photo was called a "carrier" or "mother" ship. Mrs. Hill said that it shot off smaller, "remote controlled UFOs." Another UFO, described as a "flying Y," looked remarkably like an airplane. Other

UFOs were clearly aircraft with flashing lights trailing across open film. One UFO had the letters "IUC" printed on its side (promoting perhaps the Intergalactic Umbrella Corporation). Mrs. Hill also claimed that some of the UFOs made rapid right-angle turns, an impossible feat for conventional aircraft. But it looked to me as if these sudden movements were caused by the camera being bumped.

Betty Hill's talk dragged on for over an hour, and many members of the audience were audibly groaning and giving other signs of displeasure. After she finally finished showing her blurry UFO slides, one man, whom I did not recognize, directed a hostile question to Moseley, asking what precautions had been taken to prevent the audience's time from being wasted on such "rubbish." This man apparently was not aware that Betty Hill's testimony is a major pillar of "UFO evidence." Some members of the audience, however, jumped to Betty Hill's defense. Another question was directed to Stanton Friedman, asking in a highly skeptical vein why it is that Betty Hill sees all these dozens of UFOs in New Hampshire but no one else does. Friedman's answer was entirely supportive of Mrs. Hill: she spends more time looking up at the sky, etc. He did not in any way express any *public* doubt or reservations about these many sightings, although he *privately* says he does not believe her claims of recent sightings. Such inconsistency is necessary to Friedman, since he derives his income from pro-UFO lectures and writings, and the Hill case is his best evidence for the reality of extraterrestrial visitors.

The evening program began with Charles Berlitz, author of *The Bermuda Triangle*. Berlitz mentioned the celebrated underwater pyramid that he has supposedly found in the Bermuda Triangle. He did not mention the $10,000 challenge of CSICOP fellow Larry Kusche (*SI*, Fall 1978, p. 5). The money would have been his if he could show any convincing proof of the existence of this supposed pyramid. (He could not.) Berlitz focused his talk on the famous "Philadelphia Experiment," in which a Navy ship and its crew were reputedly teleported from Philadelphia to Norfolk and back. The newly published Doubleday *Encyclopedia of UFOs* (edited by Ronald Story and H. Richard Greenwell) contains an article under "Allende Letters" stating that the entire "Philadelphia experiment" was a confessed hoax.

—Robert Sheaffer

Robert Sheaffer

UFO "abductee" Betty Hill, with a sculpture of her alleged alien captor.

Nonexistent psi-gap

Uri Geller, the former psychic star, is currently on a tour doing his Chicken Little impression, warning the West that Soviet scientists are surging ahead in psi research and proclaiming a dangerous "psi-gap." The facts are somewhat less alarming.

The Soviet concentration on para-research took a steep dive back in 1978, after an expensive and extensive effort indicated that money and time was being wasted. August Stern, a Soviet parapsychologist, worked along with 60 colleagues for over two years trying to chart various forms of energy — including those believed to be responsible for ESP — in a manner similar to the way the elements have been arranged in the Mendeleev periodic table. He found nothing useful at all in his attempts.

The Soviet team applied electric shocks to new-born kittens to see if their mothers, isolated from them, would react. They watched people via TV monitors to observe the effect of beaming ESP signals at them. They even put bacteria on microscope slides, separating them effectively, to see whether they could transmit disease by means of photons through the barrier! The experiments were abandoned, in spite of the limitless funding available.

The press services, in covering these developments, found yet another way to confuse the issue by reporting that the Soviets were "trying to find a physical basis for psychic energy, or PSI particles, as they are called." The "psi" particle is in no way connected with the claims of the paranormalists but is a part of legitimate particle physics.

The "psi-gap" is a fiction. The sky is *not* falling.

* * * * *

Note: Jack Anderson's syndicated column for January 9, 1981, reported that the U.S. Defense Department has a top-secret "psychic task force" hard at work trying to develop "psychotechtronics," or ESP weaponry, to counter the supposed Soviet threat. The Defense Department spent at least six million dollars last year researching such "psychowarfare," Anderson reported. This Pentagon "ju-ju" team, as Anderson called it, had better have some second thoughts before wasting more of the taxpayers' money in pursuit of the psychic Easter bunny.

—*James Randi*

That was the year that wasn't (again)

We continue to think psychics' predictions are more interesting to read *after* the period for which they were made, rather than before. In keeping with our tradition of serving the public on these matters, we hereby offer some of the predictions made in the article "Ten Top Psychics Reveal Their Predictions for 1980," which appeared in the October 30, 1979, issue of the national weekly tabloid *Star*:

> Senator Howard Baker will be our next president.
>
> President Carter will announce at the last moment that he will not run for reelection.
>
> Friction between the Vatican and a communist regime in Italy will lead to a siege of the Vatican and the exiling of the pope.
>
> Space scientists [will] find evidence of a long-dead civilization on a planet in the solar system.

We will make contact with aliens from outer space during 1980.

NASA scientists will invent a machine that will enable us to overcome the force of gravity.

Johnny Carson will begin a new Tonight Show on another network.

Steve McQueen will make a tremendous comeback. . . .

—*K.F.*

FCC rules UFOs aren't of "public importance"

The Federal Communications Commission has concluded that UFOs are not an issue of "public importance." The subject therefore fails to meet the criteria under which distortions by the broadcast media could come under FCC purview.

The specific case concerns a radio program entitled "The UFO Report," broadcast for five consecutive days at 5:10 P.M. in April of 1980 on station KRKK in Rock Springs, Wyoming. Lee Roger Taylor, Jr., an assistant professor in the Division of Humanities and Fine Arts and the Division of Mathematical Sciences at Western Wyoming College in Rock Springs, filed a four-page complaint with the FCC about the series.

The series featured, among other things, an interview with psychologist Leo Sprinkle on the characteristics of extraterrestrial vehicles and their occupants, an interview with an alleged psychic who claimed to be in mental contact with extraterrestrial vehicles and their occupants, and an interview with a person who alleges he has had physical contact with "flying saucers" and that their occupants told him where to dig a well.

Taylor said the series, "in a pseudo-documentary format," was an "entirely biased presentation representing the so-called UFO phenomena as extraterrestrial flying saucers." The information it presented "was purely speculative fiction, totally lacking the weight of even the barest minimum of scientific evidence."

Taylor documented the fact that the station offered no disclaimer in any form. The program was presented as a news documentary, he said. "This was underscored by the fact that the series followed immediately — without a commercial break — on the evening news." One of the station's regular newscasters made each presentation.

When Taylor requested an opportunity to offer an opposing view "and/or scientific explanations for the phenomena and events described," the newscaster "flatly refused," said Taylor. "He then, within 5 minutes of our discussion, went on the air and sarcastically stated, 'Somebody wants to give an opposing view [to the UFO series] . . . I can't believe it.' "

The station manager said the series was classified as entertainment but acknowledged that because of the way the programs were handled some listeners may not have been able to distinguish the difference between the series and a regular news broadcast. Still, the manager failed to fulfill promises to offer equal time.

"In sum," Taylor concluded in his complaint, "the documentary-like format of 'The UFO Report,' containing unsupported and unscientific information, together with its total lack of objectivity and its refusal to allow the presenting of the opposing scientific view, constitutes a serious disservice to the listening public in this area." He called the production the "worst kind of mindless sensationalism" and a "callous exploitation of human gullibility" and

urged the FCC to investigate the matter and consider it when the station next requests renewal of its license.

After several exchanges, the FCC finally officially notified Taylor, on October 8, 1980, that it could take no action on his request. Stephen F. Sewell, acting chief of the Complaints and Compliance Division, noted that under the Fairness Doctrine "if a licensee presents one side of a controversial issue of public importance, it has an affirmative obligation to afford a reasonable opportunity for contrasting viewpoints in its overall programming." But it decided UFOs are not such an issue.

"The subject of UFOs appears to be controversial because numerous theories exist as to their substance and origins," said Sewell. Yet the complaint failed to establish that "belief in UFOs is of public importance," he said.

"An issue must be more than noteworthy in order to satisfy the 'public importance' portion of the Commission's definition: it must also be the subject of vigorous debate with substantial elements of the community in opposition to one another. In other words, does the issue involve a social or political choice, and will the outcome of that choice have a significant impact on the community or its institutions?

"Each of your assertions regarding the UFOs fails to meet the Commission's criteria for establishing it as a controversial issue of public importance. Therefore, we can take no action."

Taylor also sent the complaint to the National News Council but was told it has a policy against considering complaints that are also being pursued in legal channels.

Taylor said he had felt "rather like Don Quixote fighting the windmills — only somewhat more at a loss." Yet it is interesting that once again an agency has looked into the matter and decided that UFOs are hardly the issue of cosmic importance that many UFO enthusiasts make them out to be.

—Kendrick Frazier

Analysis of a "UFO" piece

"The UFO which exploded over a vast, desolate timber property in Queensland, Australia, scattered its intricate, eerily beautiful wreckage for several kilometers." So begins the preface of a recent book published in Australia entitled *Alien Honeycomb: The First Solid Evidence of UFOs*. The book was co-authored by John Pinkney, a columnist who claims to have been a UFO researcher for more than two decades, and by Leonard Ryzman, another UFO researcher.

According to the book's preface: "Most of the pieces [of the UFO] were collected by officers from the Royal Australian Air Force, which, without public announcement, immediately dispatched the material to the Pentagon laboratories for testing." This "global conspiracy" might have succeeded in withholding the "truth" from the public "but some of the wreckage was retrieved by private investigators — among them, the authors . . . The Queensland explosion has opened new vistas in aerial phenomenon research — chiefly because private citizens, for the first time, have managed to breach the iron wall of military secrecy that surrounds UFOs, to make fundamental new discoveries," the preface says.

"That is why, in the postscript to this book, we offer all the Queensland UFO pieces in our possession to the United Nations for further analysis. The Queensland explosion transcends the hardnosed, militaristic interests of the RAAF and the Pentagon. The gorgeous, hectically colored wreckage

retrieved from Greenbank promises to yield up scientific secrets that should be shared by all mankind," Pinkney and Ryzman wrote.

The United Nations showed no interest in analyzing the pieces of honeycomb material, despite a letter by the authors to the U.N.'s Kurt Waldheim. Pinkney and Ryzman were not willing to provide samples to others for analysis, for example, Dick Smith, head of Dick Smith Electronics, Ltd., of North Ryde, New South Wales. However, Smith was able to obtain a sample of the "alien honeycomb" from Mrs. Jean Fraser, who had reported finding the material a week after seeing a bright light in the sky in 1966, exact date uncertain.

Smith arranged to have the honeycomb analyzed by the University of New South Wales. The university's report on its analysis, dated September 25, 1980, said that the honeycomb was made with materials and fabrication techniques widely employed on this planet. The fiber-glass yarn used "has been manufactured in quantity in many countries throughout the world since the production of commercial grades began about 1938." And the report noted that "there were no unknown elements presented in the materials examined."

—Philip J. Klass

Seismic waves, not psi waves

Charles Tart, prominent parapsychologist at the University of California at Davis, appeared recently in Casper, Wyoming, to debate CSICOP members Paul Kurtz and James Randi. His opening remarks left Kurtz and Randi somewhat puzzled: Tart described how his conversion to belief in psi had been fortified when a woman friend gave him an account of a strange experience she had had. This provided a perfect opportunity to examine the tendency of believers in psi to attribute paranormal causes to some events, just because a normal, more probable explanation doesn't come to mind.

Tart described how his friend had been startled in the dead of night and had jumped out of bed with a premonition of a major disturbance of some kind. As she stood at the window wondering what it could have been, the panes shook from a heavy sound wave, the result of an explosion some 30 miles away!

How could the sleeper have been awakened by an occurrence 30 miles distant before the sound wave arrived to announce the event? A bit of high-school physics solves it for us, and I checked with a geologist present who came up with verification within a few minutes. An explosion at that distance would produce a shock wave (pressure wave) that would travel in the air and in the rock. The wave would arrive in approximately 7 seconds, while the air wave (sound wave) would take 2 minutes, 26 seconds. Thus the sleeper was probably aroused by the very substantial ground wave and had time to get to the window, aware that something had happened, only to be greeted by the window-rattling sound wave 2 minutes, 19 seconds later.

—J. R.

Selective test-selection

While testing water diviners on my recent trip to Australia, an important factor in evaluating psi research was illustrated once again. One diviner, having totally failed the tests, still stuck by his claim that his usual success average was "100 percent." Queried further, he told us that he did not count his failures in this determination, since

"obviously, when I fail, the powers aren't working at that time, and, after all, I'm counting percentages on the cases where I'm divining, not when I'm just guessing!"

Perhaps we cannot expect more than that degree of logic from an amateur, but when Dr. Arthur Hastings, one of the major figures in the Targ-Puthoff "remote viewing" investigations at SRI International viewed the critique by Kammann and Marks [*SI*, Winter 1978-79, and *Nature* 274 (1978): 680], and their attempted replication of the findings, Hastings found no difficulty in declaring that K. and M. failed because no "remote viewing" was taking place when they were testing!

We often read of highly regarded psi tests in which the method includes a set of preliminary "warm-up" sessions. How tempting it is, when the preliminaries show positive results, just to move ahead with the tests, counting these unofficial warm-ups as part of the tests proper. After all, it is obvious that "the powers" are working at that point. And how many series of tests have been aborted or not reported because "the powers aren't working at that time"? One wonders.

—*J. R.*

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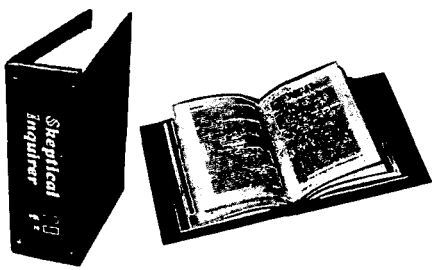

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Psychic Vibrations

"Visitors From Spirit World Give Dying Mae West the Will to Live," the *National Enquirer* reported. The spirits were quoted as saying: "Mae, you've got to fight back. Your time hasn't yet come to leave this world." That was in the issue of November 4, 1980. Mae West died on November 22, 1980.

* * * * *

The well-known "psychic" Irene Hughes predicted for the *Weekly World News* (October 21, 1980) that "Jimmy Carter will be reelected after a landslide victory over Ronald Reagan." In that same article, we find Shawn Robbins, another major "psychic," predicting that Jane Fonda will shock the world by running for office as a Republican and that "amazed scientists will discover the wreckage of a UFO in the Mount St. Helens crater — and an astonished nation will learn that the craft caused the volcano to erupt."

* * * * *

In the Fall 1980 issue we reported UFO entrepreneur Timothy Green Beckley's claims that a woman was allegedly abducted to the back side of the moon, where she was "implanted with outer space semen." An alert reader has directed us to the original source document for that incident. The story first appeared in the Dacron, Ohio, *Republic-Democrat*, a fictitious newspaper whose parent publishing company is the satirical humor magazine *National Lampoon*. Beckley claims he got the story from the *Toronto Sunday Sun*. He did give the correct date of the article, however, but he deleted a reference to the nonexistent town of Dacron and modified the name of the supposed victim — from an unprintable vulgarism to a name that looks as if it might be German.

* * * * *

John G. Fuller is the author of *Incident at Exeter*, *The Ghost of Flight 401*, and numerous other such books. John and his wife, Liz, using a Ouija board, apparently contacted a crew member who died in the 1972 crash of Flight 401 near Miami. Now Liz Fuller is making great progress in developing her psychic talents. The *National Enquirer* reports that when the Fullers were in the Himalayas researching yet another book on the paranormal she began writing proverbs. "It just happened," she said. "It was my handwriting but it wasn't me. It was signed 'Franklin.' " When she returned home, Liz showed examples of her automatic writing to certain unnamed scholars, who sup-

posedly found that they bore a resemblance to the works of Benjamin Franklin. Mrs. Fuller may have opened up a whole new avenue of research for historians!

* * * * *

Remember the fuss over the TV movie "Death of a Princess," which dramatized the execution for adultery of a Saudi Arabian princess? No need to worry, says the well-known "psychic" Sybil Leek; the princess is still alive. A poor Bedouin girl was substituted for the condemned adulteress, says Leek, and the real princess is now living in Paris. While watching the TV drama, Leek told the *Globe*, she "began getting vibrations from the real-life execution." Further research revealed that "the astrological signs all pointed to money changing hands, indicating a girl being sold as a sacrifice."

* * * * *

Fate magazine's Curtis Fuller quotes an interesting statement made by John Beloff, described as "one of the foremost thinkers in parapsychology." Beloff is becoming increasingly convinced that no physical explanation for alleged psi phenomena will ever be possible. Many scientists believe the same thing, of course, but Beloff is no skeptic; he holds that this shows not the implausibility of alleged psi phenomena but that "not everything in nature can be explained in physical terms." Beloff recommends, however, that parapsychologists retain as a "protective camouflage" the appearance of searching for a new set of physical laws. Says he, "So long as it is thought that we are engaged in the same game as the physicists and other natural scientists, there is more hope that we shall be given funds and facilities to pursue our researches."

* * * * *

The list of "authentic" monsters continues to grow. The Associated Press reports that Roy Mackal, a research associate at the University of Chicago and a well-known investigator of the alleged monster at Loch Ness, believes that dinosaurs may still be alive in a little-explored part of Africa. Having analyzed both historical and first-hand accounts, Mackal says, "Our conclusion at this time is that the reports refer to real animals, but they may be rare or even extinct now." Mackal and his associate James Powell believe that the last of the dinosaurs may be a smaller cousin of the giant brontosaurus. They say that they plan to visit the Congo and Zaire next August to look for the animals.

—Robert Sheaffer

Hypnosis and UFO Abductions

Hypnosis can encourage fantasy and implant pseudo-memories. Its validity in UFO abduction cases is questionable.

Philip J. Klass

Nearly two hundred persons now claim to have been abducted by "UFO-nauts," taken aboard a flying saucer, typically for a physical examination, and then safely released, and the number of such tales is mushrooming. If extraterrestrials are indeed visiting the earth, their curiosity is understandable; but one might expect that they would carry a few earthlings back to their native planet for a more useful dissection, rather than simply repeat the superficial examinations reported.

If only one of the alleged abductees had managed to bring back a single souvenir, the UFO question would be resolved incontrovertibly. But since this has not occurred, UFO proponents rely on regressive hypnosis as their principal tool to substantiate the tale of abduction, and almost invariably it seems to confirm the account. The key question is whether hypnosis is really an effective "lie-detector."

James A. Harder, one of the principal practitioners of hypnosis for UFO incidents, claims that "it is impossible to lie under hypnosis." Harder, director of research for APRO, one of the nation's oldest UFO organizations, is a professor of civil engineering. On the strength of Harder's use of hypnosis, he has endorsed the alleged UFO abductions of Charles Hickson and Calvin Parker in Pascagoula, Mississippi, in 1973 and Travis Walton in Arizona in 1975. (My own investigations indicate both incidents are hoaxes.)

Philip J. Klass is chairman of CSICOP's UFO Subcommittee.

The first known use of regressive hypnosis in connection with a reported UFO abduction occurred in 1963-64. The case involved Betty Hill and her late husband, Barney, who were treated by Dr. Ben Simon, a respected Boston psychiatrist. Simon achieved fame in psychiatric circles during World War II when he used hypnosis to successfully treat military personnel suffering battle-induced psychoses at the U.S. Army's Mason General Hospital, where he was chief of neuropsychiatry.

When the full story of the Hill case was made public in the fall of 1966, in two widely read articles in *Look* magazine and in a book entitled *Interrupted Journey*, leaders of the UFO movement generally were *not* impressed with the tale. Possibly this was because the story was too incredible by the movement's then current standards. Simon sharply disagreed with the conclusions of the book's author, and in a brief introduction he wrote for the book he cautioned that hypnosis is not a "magical and royal road to the Truth."

Shortly after the *Look* articles were published I interviewed Dr. Simon, and he told me he was certain that the alleged UFO abduction was fantasy, not fact, although he was equally certain that the Hills had seen a bright light in the night sky that they had found frightening. To demonstrate the basis for his conclusions, Simon played some tapes for me of Barney reliving under hypnosis his experience of looking at the bright light. I could hear the terror in Barney's voice, and Dr. Simon told me he had never before had a patient become so agitated under hypnosis.

But when we listened to Betty and Barney reliving the alleged experience of being taken aboard a flying saucer by strange-looking creatures, their voices were relaxed and casual, as if they were describing a visit to a neighborhood shopping center. Simon cited numerous other reasons for his conclusion that the tale of abduction was fantasy. Much later, when he appeared on NBC-TV's "Today" show on October 20, 1975, prior to that network's two-hour pseudo-documentary on the Hill incident, Simon was asked whether he believed a UFO abduction had really occurred. He responded that "the abduction did *not* happen," and he characterized it as "fantasy."

When so experienced a practitioner of hypnosis as Simon rejects the idea that simply because a tale of UFO abduction is told under hypnosis it must be true, one might expect that far less experienced practitioners would be cautious in its use for this purpose. But this has not deterred R. Leo Sprinkle, an APRO consultant, who is the leading practitioner of hypnosis in UFO-abduction cases. (Sprinkle, a psychologist, is director of counseling and testing at the University of Wyoming.) In a paper presented to the American Psychological Association in Toronto, on August 28, 1978, Sprinkle reported that he had used hypnosis on 25 persons and had

"obtained information . . . which supports their claims of 'abduction' experiences." He said he was inclined "to accept, tentatively, the claims of UFO abductions as 'real.' " He added: "I do not know if these 'abductees' have experienced physical abduction, or whether they have experienced 'out of the body' events [an alleged psychic phenomenon]. In either case, the experiences seem 'real' to the 'abductee.' "

The crucial issue is not whether the tale "seems real" to the subject but *whether the alleged abduction actually occurred.* On January 23, 1977, I wrote to Sprinkle: "To your knowledge, has anyone conducted controlled experiments to evaluate the effectiveness of regressive hypnosis in determining whether the subject is intentionally trying to perpetrate a hoax or a falsehood?" I added that if such experiments had not been conducted it seemed to me that this should be done "before UFO investigators invest any more time in its use as a means of trying to sort out reality from nonreality."

Sprinkle replied promptly, saying that when he first began to experiment with the use of hypnosis on abduction claimants in the mid-1960s he was unconvinced that an abduction had really occurred. But with further use of hypnosis, Sprinkle said, he "began to recognize that there was no way for me to 'know' whether the UFO witness did or did not experience an abduction." Then Sprinkle made a remarkably candid admission: "Now I have persuaded (conned?) [*sic*] myself that . . . the apparent abduction experiences are 'real'; at least they are real in the minds of these persons . . . I accept these experiences as 'real,' although I'm not in a position to determine the 'level of reality.' " In my reply, I sought clarification of "level of reality." Sprinkle tried to explain that there are *many* "levels of reality," a concept my simple mind was unable to grasp.

In May 1977, the UFO movement's growing reliance on hypnosis to support tales of UFO abductions was shaken by a paper published by Alvin H. Lawson, a professor of English at California State University, Long Beach. The paper was entitled: "What Can We Learn from Hypnosis of Imaginary 'Abductees'?" Lawson had a long-standing interest in UFOs and had offered courses in UFO literature. He reported on an experiment in which imaginary UFO abductions were induced hypnotically in a group of subjects who were then questioned about their experience. The hypnosis was administered by William C. McCall, an M.D. with clinical experience in its use.

Not only were the subjects able to improvise answers about what had happened to them aboard the imaginary flying saucer, Lawson reported, but their stories "showed no substantive differences" from tales in the UFO literature by persons who claimed to have actually experienced an abduction. This prompted Lawson to observe: "The implications of the study for

"MAN ABDUCTED BY UFO FOR 4TH TIME ... Truth Serum and Hypnosis Confirm His Story"

—*National Enquirer*, January 15, 1980

"NBC-TV Movie Based on Tapes Made Under Hypnosis: ABOARD A FLYING SAUCER ... The Incredible Story of Two People Who Believe They Were 'Kidnapped' by Humanoids in a Spacecraft"

—*National Enquirer*, August 19, 1975

"Hypnosis tests show that UFO 'kidnap victims' are telling the truth, say doctors"

—*The Star*, November 7, 1978 (with photo of Dr. R. Leo Sprinkle)

"Under hypnosis, housewife tells of abduction by aliens"

—*Houston Chronicle*, April 29, 1979 (the Betty Andreasson case)

"Hypnosis & Truth Test Confirm Man's Story: I WAS ABDUCTED BY A UFO"

—*National Enquirer*, May 8, 1979

future hypnotic regression of Close Encounter cases, and for abduction cases now deemed of the highest credibility, are unclear at this time."

The results of this experiment, and Lawson's conclusions, were attacked sharply by Harder in the September 1977 issue of the *APRO Bulletin*. Harder said that, while Lawson admitted that the experiment did not prove that all UFO abduction reports were imaginary, "his paper may well lead naive readers to think that there is a strong case that they all are [imaginary]." Harder criticized Lawson's paper for pointing out the similarities between the stories told by "real" and "imaginary" abductees, while failing to point out "a very important difference." That difference, Harder claimed, "was that the 'real' abductees were convinced that their experiences were real whereas the 'imaginary' abductees were not."

Lawson offered a revised paper on the same experiment at the August 28, 1978, meeting of the American Psychological Association. He stuck by his original report that there were "no substantive differences" in the accounts given by "real" and "imaginary" abductees. But he added that "despite the many similarities, there are crucial differences — such as alleged physical effects and multiple witnesses — which argue that UFO abductions are separate and distinct from imaginary and hallucinatory experiences." However, Lawson also warned that "one should be cautious about the results from hypnotic regression in UFO case investigations ... A witness can lie, or believe his own lies, and thus invalidate any investiga-

tion. A more common result may be that hypnotized witnesses subtly confuse their own fantasies with reality — without either the witness or the hypnotist being aware of what is happening." It is clear that Lawson is much more knowledgeable about the limitations and pitfalls of hypnosis than Harder, who so often employs this technique.

The obvious lessons to be drawn from the Lawson/McCall experiments and papers have been largely ignored by the leaders of the UFO movement. For example, J. Allen Hynek, scientific director of the Center for UFO Studies, has strongly endorsed the tale of Mrs. Betty Andreasson, told under hypnosis, who claims not only to have gone aboard a flying saucer but also to have flown to its native planet. In the foreword to a book about the Andreasson case, Hynek wrote: "In the past, I frankly would not have touched an invitation to write the foreword for a book treating 'contactees,' abduction, mental telepathy, mystical symbolism, and physical contact and examination by 'aliens.' But across the years I have learned to broaden my view of the entire UFO phenomenon. Those who still hold that the entire subject of UFOs is nonsense will be sorely challenged if they have the courage to take an honest look at the present book."

Martin T. Orne, past president of the International Society of Hypnosis and director of the Institute of Pennsylvania Hospital's unit for experimental psychiatry, is an internationally recognized authority on hypnosis. In a paper published in the October 1979 issue of the *International Journal of Clinical and Experimental Hypnosis*, entitled "The Use and Misuse of Hypnosis in Court," Orne completely demolishes the basic premises upon which Harder, Sprinkle, and other UFOlogists have operated in using hypnosis in an effort to substantiate tales of UFO abductions. While Orne does not discuss the misuse of hypnosis in UFO cases, except for one oblique reference, it is obvious that his warnings and recommended safeguards apply to UFOlogy as well as to forensic use.

Orne notes that the courts "have recognized that hypnotic testimony is not reliable as a means of ascertaining the truth," and he says this view "is supported by scientific data." He cites experiments showing that "it is possible for an individual to feign hypnosis and deceive even highly experienced hypnotists ... Further, *it is possible for even deeply hypnotized subjects to willfully lie*" (emphasis added). This flatly contradicts Harder's self-serving claim.

Orne warns: "We should keep in mind that psychologists and psychiatrists are not particularly adept at recognizing deception." (Surely this also applies to a professor of civil engineering.) "We generally arrange the social context of treatment so that it is not in the patient's interest to lie to us ... As a result, the average hotel credit manager is considerably more adept at recognizing deception than we are." Orne acknowledges that

"military psychiatrists and other health professionals who are required to make dispositional judgments on a daily basis do become adept at recognizing manipulation and deception." But Orne says that relatively few "who are experienced in the use of hypnosis have had this type of background. Consequently, they have little experience or concern about being deceived or used."

Orne cautions: "Hypnotic suggestions to relive a past event, particularly when accompanied by questions about specific details, puts pressure on the subject to provide information for which few, if any, actual memories are available. This situation may jog the subject's memory and produce some increased recall, *but it will also cause him to fill in details that are plausible but consist of memories or fantasies from other times"* (emphasis added). He adds: "It is extremely difficult to know which aspects of hypnotically aided recall are historically accurate and which aspects have been confabulated."

"There is no way, however, by which anyone — even a psychologist or psychiatrist with extensive training in the field of hypnosis — can for any particular piece of information determine whether it is an actual memory versus a confabulation *unless* there is independent verification," Orne states. He cites experiments by others that show that "free narrative recall will produce the highest percentage of accurate information but the lowest amount of detail. Conversely, the more an eyewitness is questioned about details, the more details will be obtained — but with *a marked decrease in accuracy"* (emphasis added). (Examination of transcripts of hypnosis sessions with "abductees" reveals that great pressure was applied for details rather than allowing the subject to use free narrative.)

Orne's paper suggests that the use of hypnosis by pro-UFO investigators can generate what he calls "pseudo-memories," which may enable a subject to tell a convincing story later when not under hypnosis. Such "pseudo-memories can and often do become incorporated into the individual's memory store as though they had actually happened . . . If a witness is hypnotized and has factual information casually gleaned from newspapers or inadvertent comments made during prior interrogation or in discussion with others . . . many of these bits of knowledge will become incorporated and form the basis of any pseudo-memories that develop."

One of Orne's warnings is especially appropriate for hypnotic interrogations conducted by Sprinkle and Harder, both of whom lean strongly to the hypothesis that the earth is being visited by extraterrestrial craft. Orne writes: "Furthermore, if the hypnotist has beliefs about what actually occurred, it is exceedingly difficult for him to prevent himself from inadvertently guiding the subject"s recall so that he [the subject] will eventually 'remember' what he, the hypnotist, believes actually happened."

(During my own investigation into the Travis Walton "abduction" case, I talked with Jean Rosenbaum, a Durango, Colorado, psychiatrist who was brought into the case and was in Scottsdale, Arizona, when Harder used hypnosis to interrogate Walton to probe for details of the incident. Rosenbaum told me that Harder's "interviewing techniques are very interesting in that all of his questions are loaded.")

Orne notes that "the more frequently the subject [describes] the event, the more firmly established the pseudo-memory will tend to become. In [conducting] the experimental demonstration, we are dealing with an essentially trivial memory about which the subject has no inherent motivations [to be untruthful]. Nevertheless the memory is created by a leading question, which, however, on casual observation, seems innocuous." Orne warns: "Hypnosis has not resulted in accurate memories but rather has served to produce *consistent memories*" (emphasis added).

After discussing these potential pitfalls, Orne proposes four important procedural safeguards. One of these is that hypnosis "should be carried out by a psychiatrist or psychologist with special training in its use." (In one instance, when Sprinkle had to return home after a hypnosis session with an "abductee" and UFO-writer Jerome Clark wanted to continue the investigation, according to Clark's subsequent article in the August 1976 *UFO Report,* Sprinkle urged Clark to "conduct hypnotic regression" himself. "Since I had never performed hypnosis before, I was dubious about the prospect, but Sprinkle had taught me the methods and said he could see no reason why Sandy [the subject] and I could not work together. So three weeks later ... Sandy and I got together in an effort to continue the interview ... Sandy fell quickly into a hypnotic trance and was able to reply quickly and easily to my questions," Clark wrote.)

Orne cautions that the hypnotist "should not be informed about the facts of the case verbally; rather he should receive a written memorandum outlining whatever facts he is to know, carefully avoiding any other communication which might affect his opinion ... It is extremely undesirable to have the individual conducting the hypnotic sessions have any involvement in the investigation of the case." (Based on this safeguard, Sprinkle and Harder would be disqualified in their UFO investigations.)

Another important safeguard recommended by Orne is that "all contact of the psychiatrist or psychologist with the individual to be hypnotized should be *videotaped from the moment they meet until the entire interaction is completed* [emphasis added]. The casual comments which are passed before or after hypnosis are every bit as important to get on tape as the hypnotic session itself. (It is possible to give suggestions prior to the induction of hypnosis that will act as post-hypnotic suggestions.)"

Additionally, Orne says: "No one other than the psychiatrist or psychologist and the individual to be hypnotized should be present in the room before and during the hypnotic session. This is important because it is all too easy for observers to inadvertently communicate to the subject what they expect, what they are startled by, or what they are disappointed by." (This recommended safeguard almost invariably is violated in UFO investigations.)

Orne also recommends that tape recordings of prior interrogations be made "because the interactions which have preceded the hypnotic session may well have a profound effect on the sessions themselves." Orne cautions that a subject may unwittingly have been given cues to certain information "which might then be reported for apparently the first time by the witness during hypnosis."

After I read Orne's paper, it was obvious that it should have been of great importance to UFOlogists who make use of hypnosis, and so I wrote to Sprinkle on March 24, 1980, seeking his reactions. He replied on April 7, saying: "Dr. Orne is an acknowledged authority on the use of hypnosis . . . However, I am sure that he would agree with the principle that 'Science' is based upon accumulated evidence of many observations, as well as upon the views of authorities." This was a curious response inasmuch as Orne had cited 37 different scientific papers and court cases to support his conclusions and recommendations.

Sprinkle said that the safeguards recommended by Orne "seem most appropriate for the forensic uses of hypnosis in court," but he questioned whether they were also applicable to UFO investigations because there is no "crime" and no "criminal" or "victim." (Sprinkle takes a curious view of "UFO abductions." He does not consider "abductees" to be "victims" even when they claim to have been taken aboard a flying saucer against their will and subjected to physical examinations. Sprinkle explained his benign views: "They do not seem to perceive themselves as 'kidnapped.' In fact, they see themselves as citizens of a 'higher civilization.'" In response to my subsequent questions, Sprinkle told me that if a member of his immediate family were kidnapped by an earthling, he *would* report it to the Federal Bureau of Investigation. But if a member of his family were kidnapped by an extraterrestrial, he would report it "to the world," *not to the FBI.*)

In subsequent correspondence, Sprinkle indicated that he had begun to use video-taping of hypnotic sessions, when the subject was willing, but other than this he indicated no plans to introduce the rigorous safeguards recommended by Orne. Sprinkle explained: "In my opinion, there are three general 'models' of UFO investigation involving hypnotic procedures. One is the 'forensic model,' as indicated by Dr. Orne; another is the 'psychotherapy model,' indicated by Dr. Simon in his work with Betty and

Barney Hill; and a third model is one of a combination of the forensic/therapeutic models, which I call the 'educational model.' "

"The educational model is based upon the view that two goals are important in UFO investigation: as much information as possible should be obtained, but not at the expense of the dignity of the individual . . . In the legal model, the search for 'truth' is intense . . . In the therapy model, the truth is less important than the personality structure and the welfare of the individual . . . The educational model is one which suggests that the individual's growth is important, but sharing that information with others can assist the individual, and others, in their educational development," Sprinkle wrote. This "educational model," he admitted, "may be considered a 'loose' model by proponents of either the therapy or legal models, because [of] the manner in which 'truth' is being explored, sought, and shared."

Asked to respond to the crucial question of whether he believed that hypnosis was of any value in determining that a UFO-abduction had actually occurred, Sprinkle replied: "I believe that the use of hypnotic techniques is helpful to UFO abductees and contactees in exploring their memories of their experiences and that it is helpful to them in assisting them to come to terms with the abductions which have occurred — in this very reality!" •

Hypnosis Gives Rise to Fantasy and Is Not a Truth Serum

Ernest R. Hilgard

We asked this noted hypnosis expert to add to Philip Klass's comments about the use of hypnosis in UFO abduction cases. —Ed.

The use of hypnotic recall as evidence in UFO abduction cases is an abuse of hypnosis. It is an abuse, first, because of the role that fantasy plays for all hypnotically responsive subjects and, second, because abundant evidence exists that fabrication can take place under hypnosis.

For example, under hypnosis I implanted in a subject a false memory of an experience connected with a bank robbery that never occurred, and the person found the experience so vivid that he was able to select from a series of photographs a picture of the man he thought had robbed the bank.

At another time, I deliberately assigned two concurrent — though spatially very different — life experiences to the same person and regressed him at separate times to *that date.* He gave very accurate accounts of both experiences, so that a believer in reincarnation, reviewing the two accounts, would have suspected that the man had really lived the two assigned lives.

These particular examples have not been published, but many similar accounts have been. For example, it has been shown experimentally that, while acting the part of a spy, a subject can hold a "cover" when posing as a citizen of another nation and in an occupation not his own. Under hypnosis, the person does not give himself away (Orne 1971).

The role of fantasy in hypnosis has been amply documented by Josephine R. Hilgard (1979).

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Ernest R. Hilgard is professor emeritus of psychology at Stanford University, a former president of the International Society of Hypnosis (1974-77), and a member of the National Academy of Sciences.

A Critical Analysis of H. Schmidt's Psychokinesis Experiments

Schmidt's random-number generator experiments cannot be considered evidence for PK

C. E. M. Hansel

In this journal, Kendrick Frazier gave details of an American Physical Society symposium at which Helmut Schmidt's psychokinesis (PK) experiments were discussed.[1] In his review Frazier quotes Ray Hyman as saying: "By almost any standard Schmidt's work is the most challenging ever to confront critics . . . His approach makes many of the earlier criticisms of parapsychological research obsolete." In my own discussion of that research I suggested that Schmidt's experiments were far from watertight and that he "may have been a careless experimenter."[2] I would now like to elaborate upon that statement.

In the case of an experiment giving very high odds against the result being due to chance, under what are claimed to be watertight experimental conditions, a demonstration of flaws in the experimental design should be sufficient to remove that experiment from the repertoire of so-called conclusive demonstrations of paranormal processes cited by parapsychologists. But parapsychologists have been loath to drop any experiment unless the experimenter or the subjects admit to having cheated. It is therefore necessary not only to point out laxity in experimental conditions but also to indicate the precise manner in which the observed scores may have arisen.

I stated that any experiment must stand on its own feet and that when analyzing an experiment it is wise *initially* to adopt the assumption that

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ESP (or in this case PK) is impossible and then see how the result could have arisen through already established processes.[3]

If this approach is adopted, the following conclusions are reached when considering Schmidt's experiments on PK in humans.

1. It would have been a simple, matter for the experimenter to bring about the observed result in the absence of PK.

2. The conditions discussed in the report do not remove the possibility of the subjects' bringing about the result.

3. Another outside person could conceivably be responsible for the result.

4. The experiments were inadequately designed in terms of standards already existing within parapsychology.

5. These criticisms apply to other experiments reported by Schmidt.

In Schmidt's PK experiments (reported in the *Journal of Parapsychology*) the subject looked at a display panel containing 9 lamps arranged in a circle, one of which was lit.[4] Approximately each second the light jumped to the adjoining position in a clockwise or counterclockwise direction according to whether a +1 or a -1 was generated by a binary random-number generator (RNG).

The claimed result of the PK experiments involved two degrees of improbability. The subjects attempted to cause the light to move in a particular direction, but their "psychokinetic influence" was claimed to affect the operation of the RNG, whose outputs were indirectly dependent upon a quantum process (i.e., the instant in time when an electron is emitted from radioactive material). The subjects were claimed to affect the radioactive material when they were attempting to affect the light whose movement was determined by the RNG.

In the *New Scientist* Schmidt reported a subsequent experiment in which two subjects, K.G. and R.R., took part.[5] They had scored consistently high and low, respectively, in preliminary tests when attempting to move the light in a clockwise direction. Each subject had to complete a prespecified 50 runs of 128 trials each; a trial was denoted by a +1 or -1 output being generated by the RNG. The result claimed was that with K.G. as subject the light moved in a clockwise direction in 52.5 percent of the total of 6,400 light jumps, whereas with R.R. as subject the light moved 47.7 percent of the 6,400 light jumps in a clockwise direction. These results were obtained consistently throughout the experiment, showing a steady increase and decrese of the accumulated deviations from the chance score (Figure 1). The odds against chance accounting for the difference in scores of the two subjects was stated by Schmidt to be 10 million to one. This experiment will be considered in view of its relative simplicity. Assuming that PK is impossible, something happened during that experiment to

create the difference in the scoring of the two subjects.

It should first be noted that the present discussion should hardly be necessary. As Martin Gardner commented: "It is impossible to evaluate Schmidt's work because he works virtually alone, and no one so far has had any access to his raw data."[6] Since at least the 1930s parapsychologists have been careful to design experiments so that the result is not dependent on the care or honesty of a lone experimenter. The Pearce-Pratt and Pratt-Woodruff experiments each contained conditions that made it impossible (or so it was assumed) for any one person to affect the result.

If we assume that the printout provided an accurate record of the experiment and that the record of the counter readings contained precisely those values indicated during the experiment, the conclusions above still apply.

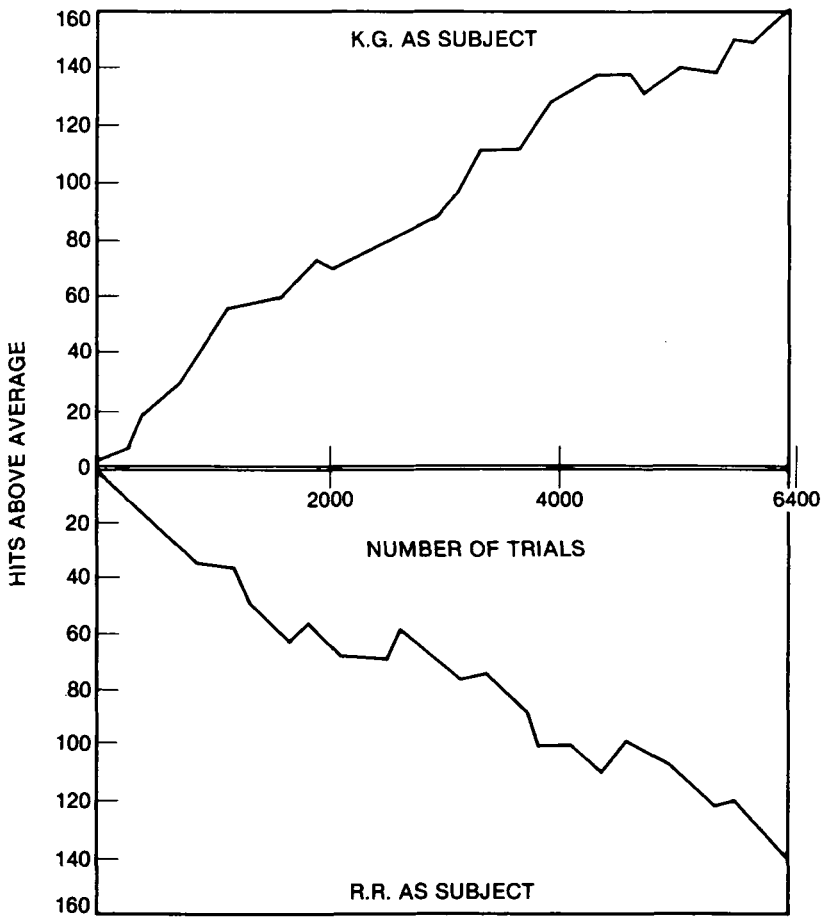

FIGURE 1. Cumulative number of hits above chance value plotted after each 256 trials.

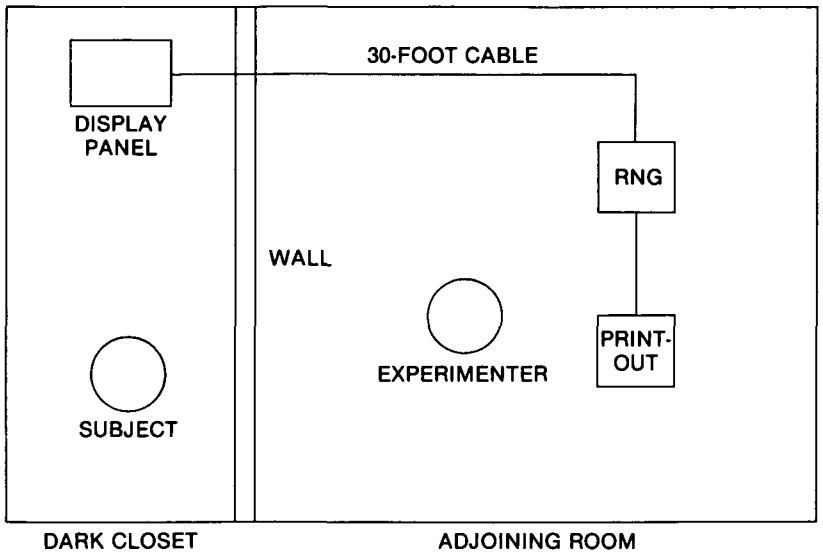

FIGURE 2. Experimental arrangements — schematic and not to scale — employed during PK experiments on humans.

The Experimenter

The experimenter would have had little difficulty affecting the experiment. He and the subject were in different rooms. The report states: "The subject sat with the display in a small dark closet, and the generator was stationed in another room, mostly as far as 20 feet away from the subject." (See Figure 2.) "The RNG and the experimenter were stationed in the room outside the closet."[7]

The experimenter was in a position to affect the printout, and he also was responsible for recording values on the counters after each run. A remarkably clear demonstration that a printout can be tampered with was provided by W. L. Levy some years later when he admitted to having done so. He was working in the same laboratory as Schmidt, and Levy states in a report that he used an RNG similar to that used by Schmidt.[8] It is even possible that Levy used the same printout employed by Schmidt in his PK experiments.

If the counters operated from the outputs of the RNG, they also would have been affected in the same manner as the printout, as discussed below.

If nonresettable counters (not mentioned in the report) were used, these would have displayed only an excess of 16 +1 outputs over 12,800 trials, owing to the fact that the one subject's deficiency of hits was

balanced by the other's surplus.

The Subject

Since the subject merely gazed at the lights on the display panel, it might at first appear that he was in no position to affect the RNG. But it is likely that he could have affected the counters and the printout, while still inside his dark closet, by shorting either the +1 or the -1 input in the display panel to the earth line according to whether he wished to produce a high or a low score. Shorting the +1 input for a total of about 6 seconds in each run would have produced a deficiency of hits similar to that obtained by subject R.R. Shorting the -1 input in the same manner would account for the surplus of hits obtained by K.G. These inputs to the display panel were connected via a cable to the outputs 0+ and 0- of the RNG. If the counters and the printout were fed from these same outputs 0+ and 0- they also would have been shorted via the cable and would have caused similar deficiencies in the record of +1 and -1 outputs recorded on the paper tape (Figures 3a and 3b).

It might be expected that the use of such a trick would have been evident to anyone watching the printout, since the jumps normally arose at "typically one second intervals," but if there were any variations in this time interval an omission would have been more difficult to detect and

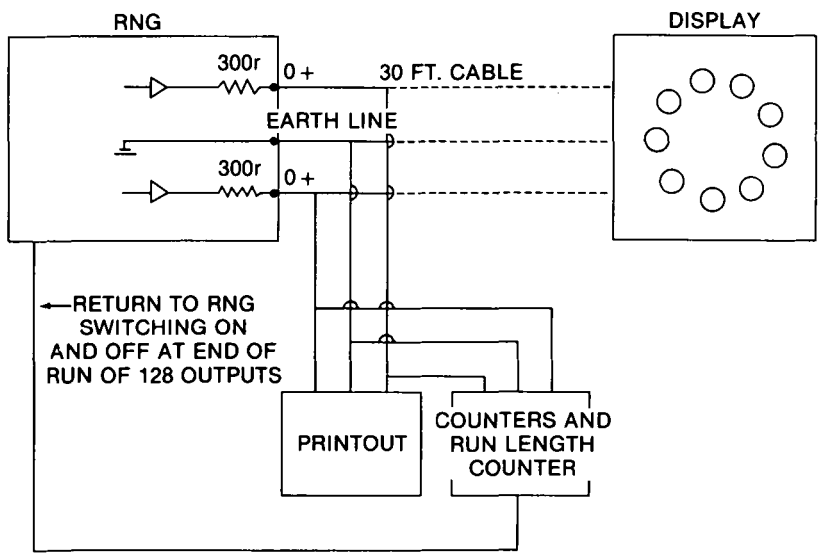

FIGURE 3a. Possible manner in which the RNG outputs were connected to the display panel, printout, and counters in the PK experiments.

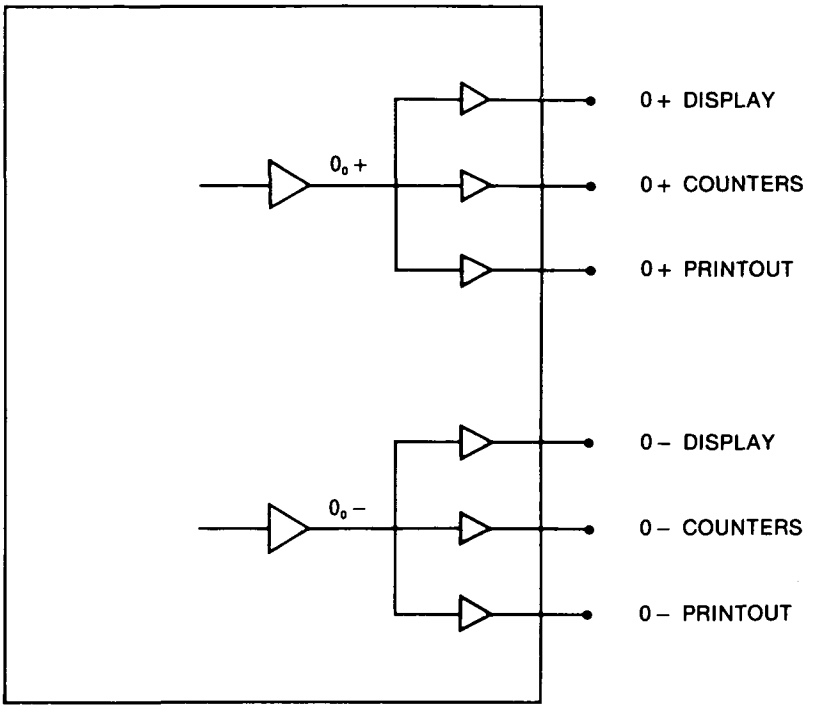

FIGURE 3b. Isolation of outputs of RNG to give independent operation of display panel, counters, and printout.

would have required constant vigilance on the part of the experimenter.

The circuit diagram of a similar type of RNG, published in the same volume of the *Journal of Parapsychology*, does not contain means for stopping the RNG at the end of a run.[9] If the number of outputs (128) in each run was determined by additional circuitry running from the RNG outputs that stopped the RNG after each 128 outputs, the length of the run as recorded on the printout and on the counters would not be affected through shorting either 0+ or 0− outputs to the earth line.

Another feature of the RNG circuit given in the *Journal of Parapsychology* is that the 0+ and 0− outputs are connected to the output transistors via 300-ohm resistors, so that shorting an output would not damage the output transistor in the machine (see Figure 3a) or affect the other output.

Figure 3b shows how the various outputs could be isolated. With this modification, only the experimenter would be able to affect the three outputs by shorting outputs 0_0+ or 0_0− to the earth line.

Other Persons

Anyone conversant with the circuitry could have brought about the same result by tapping the 30-foot cable connecting the display panel to the RNG. Precautions do not appear to have been taken to ensure that connections to the external apparatus were sealed and checked after each experiment.

General Design Features

Checks on the efficient operation of the RNG were made by letting the machine run "frequently unattended" to confirm the absence of any systematic bias. It is not stated when these checks were made during the course of the experiment, and the system employed is far from satisfactory.

In an experiment of this nature it is necessary to compare the results of an experimental series with those of a control series in which the factor being investigated is absent but which is similar to the experimental series in other respects. Had a control series been incorporated in the design in a suitable way, many of the difficulties discussed above would have been overcome. Thus each run could have been made as either (1) an experimental run or (2) a control run following the toss of a coin or some other more sophisticated randomization procedure. The subject would not be "willing" the light to move, or he would aim at moving the light in the opposite direction, during control runs.

To control the experimenter, only the subject or an independent investigator should have a record of which condition was used for each run. It would have been a simple matter to incorporate control series in order to safeguard against trickery on the part of each person involved in the experiments and to check the efficient operation of the RNG.

Schmidt should have realized that some critics would regard the processes being investigated not so much as highly unlikely but as utterly ridiculous. His results might be sufficient to convince his fellow parapsychologists, but critics would wish to establish what went on during the experiment to bring about the result. It was either extreme carelessness in collecting data or carelessness in employing methods and procedures that permitted alternative explanations.

Conclusions

1. The conditions reported by Schmidt did not eliminate the possibility of trickery.
2. Whether or not a trick was used by the experimenter, by a number

of subjects, or by an outside person, the experiments cannot be considered as providing evidence for PK.

3. Further information about the apparatus might eliminate the possibility that any of the subjects or any outsiders were involved.

4. Unsatisfactory features are also evident in other experiments reported by Schmidt, both in the experimental design and in the apparatus used.[10] These other experiments should be considered in relation to features also present in the PK experiments.

Notes

1. Kendrick Frazier, "Schmidt's Airing at the A.P.S.," SKEPTICAL INQUIRER, Summer 1979, pp. 2-4.

2. C. E. M. Hansel, *ESP and Parapsychology: A Critical Re-evaluation*, Prometheus Books, Buffalo, N.Y., 1980, pp. 220-233.

3. See Note 2.

4. Helmut Schmidt, "A PK Test with Electronic Equipment," *Journal of Parapsychology* 34 (1970): 175-181.

5. Helmut Schmidt,"Mental Influence on Random Events,"*New Scientist* 20, June 24, 1971, pp. 757-758.

6. See Note 1.

7. See Note 4.

8. W. J. Levy, Jr., and Anita McRae, "Precognition in Mice and Birds," *Journal of Parapsychology* 35 (1971): 121.

9. Helmut Schmidt, "A Quantum Mechanical Random-Number Generator for Psi Tests," *Journal of Parapsychology* 34 (1970): 219-224.

10. Helmut Schmidt, "Anomalous Prediction of Quantum Processes by Some Adults," *Boeing Research Laboratory Document*, D1-82-0821, February 1969. ●

Further Comments on Schmidt's PK Experiments

Alternative explanations are abundant

Ray Hyman

C. E. M. Hansel begins his provocative "Critical Analysis of Helmut Schmidt's Psychokinesis Experiments" by quoting me. As part of my talk to the American Physical Society, I said, "By almost any standard Schmidt's work is the most challenging ever to confront critics . . . His approach makes many of the earlier criticisms of parapsychological research obsolete." In seeming contrast, Hansel quotes his own assessment that Schmidt's work is far from watertight and that he "may have been a careless experimenter." That assessment was made in his recent book *ESP and Parapsychology: A Critical Re-evaluation.* The implication is that Hansel and I disagree in our judgment of this work.

Surprisingly, both Hansel and I agree about the bottom line. We both conclude that Schmidt's experiments with random-number generators do not provide an adequate case for the existence of psi (PK, ESP, etc.). So in this very fundamental sense we are in the same camp. Neither of us believes that a scientific case has been made for the existence of psi.

However, we differ in what is probably an even more fundamental attitude toward such work. We differ both on how we *justify* our skepticism and on how we *proclaim* it to the world. I try to justify my position in terms of how well the evidence fulfills explicit standards of reliability and coherence. Hansel builds his case almost entirely upon the possibility of trickery having taken place during the research process. These two modes

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of argument are quite different, as I will try to make clear. Furthermore, I try to present my arguments in such a way that they can be constructive and possibly help researchers to get closer to the truth. Hansel puts himself into an adversary role — one that obviously invites hostile responses from the parapsychologists. Another way to draw this distinction is in terms of the objectives of our critiques. Are we skeptically viewing the work of the parapsychologists with the hope of discovering what is truly going on? Or are we engaged in a struggle in which one of the sides must emerge victorious and the other must be vanquished?

Before I continue with my elaboration of these points, I should point out that Hansel and I are not responding to the same set of experiments. For some unexplained reason, Hansel confines his critique to the first two years of Schmidt's program. He cites no work conducted by Schmidt or others with random-number generators after 1971. Yet Schmidt and other parapsychologists have been publishing such work right up to the present. So my evaluation of Schmidt's work is based on experiments conducted by him and others over a ten-year period. The fact that other experimenters have claimed varying degrees of success with machines of the Schmidt type, for example, changes the import of some of Hansel's criticisms.

Kendrick Frazier's summary of both Schmidt's and my talks at the APS symposium on physics and parapsychology (New York, January 30, 1979) is excellent, and I urge those who are interested in the basic points made to read that account in the Summer 1979 issue of this journal. Schmidt and I were allotted 45 minutes each for our presentations, and, as Frazier indicates, the written account of my talk covers 28 typewritten pages. Frazier fully summarizes my general criticisms of parapsychological work, including that of Schmidt. But he obviously could not fully detail my specific critique of Schmidt's work. As I will point out, this detailed critique includes some of the points made by Hansel. In addition, it mentions others.

Hansel was not the only reader of Frazier's summary to focus upon the good things I said about Schmidt's work. Many reporters and readers also picked this up as the apparent theme of my talk. And Hansel is not alone in treating my position as in seeming contrast to his own. Theodore Rockwell, in his review of Hansel's book in a recent issue of the *Parapsychological Review*, juxtaposes our two assessments in such a way as to maximize the apparent divergence in our views. Neither Hansel nor Rockwell seems to realize that my talk was a rebuttal to Schmidt's claims. I was on the panel to represent the skeptical viewpoint, and the thrust of my remarks was to warn the physicists to take Schmidt's and other parapsychologists' claims with a grain of salt.

In the course of carrying out this task I also pointed out those grounds

for listening to the parapsychologists, and I tried to make it clear that current work in parapsychology cannot be dismissed by the stock criticisms that were generated during Rhine's early work. Instead, the work of Schmidt and his contemporaries has to be evaluated on different and more sophisticated grounds. For example, criticisms that were relevant to the hand-shuffling of cards, to sensory leakage, to mistakes in the hand-recording of scores, and to the misuse of standard statistical tests no longer apply — at least not in the same way.

So one proper context within which to interpret my statement about Schmidt's work making "many of the earlier criticisms of parapsychological research obsolete" is that it employs a new technology and a new level of sophistication regarding randomization of targets, recording the data, sensory leakage, and sources of experimenter bias. This does not by any means make it beyond criticism. In fact, I supplied a number of specific criticisms.

The other context is in terms of the overall objectives of my contribution to the symposium. Schmidt, a quantum physicist, talked about his own research and theories on psychokinesis. My task was to react responsibly and skeptically to Schmidt's presentation. I did this by first placing this specific research into the larger context of physicists working in psychical research. I reviewed 125 years of involvement in such research by physicists. Among other lessons that emerged from this historical survey was the important one that training in physics was not highly relevant to investigating claims of psychical powers. Expertise in one field does not necessarily transfer to another — especially one so unstructured and untidy as parapsychology.

Schmidt's particular program introduced some space-age technology and new levels of sophistication in instrumentation, selection, and presentation of targets, in recording the data, and in theory. His reports showed care and attention to certain details that were a step forward. For these reasons I commended what he had accomplished so far. And I felt that his work would not be dismissed out of hand — at least not in terms of the type of objections that have been leveled at more traditional forms of parapsychology.

On the other hand, I pointed out reasons for reserving judgment and being somewhat dubious about the results at this time. Some of these reasons were general in terms of the lessons derived from the overall history of parapsychology. Beginning with the founding of the Society for Psychical Research in 1882, each generation of parapsychologists has put forth its current candidates for the proof of ESP or PK. These candidates were particular experiments or experimental programs that, allegedly, ought to have convinced any rational person who fairly assessed the

results. In 1882, for example, the candidates included the telepathy experiments with the Creery sisters and with Smith and Blackburn. Both these candidates were dropped from the pool of evidence when the sisters were later caught and then confessed to fraud and when Blackburn explained how he and Smith had employed a code to outwit the researchers. But they were quickly replaced with other candidates. In the 1940s and 1950s the Soal-Goldney experiments with Shackleton and Mrs. Stewart were the centerpieces of the candidate pool. Partly as a result of Hansel's suspicions, these experiments eventually left the pool. Today the pool features the Ganzfeld experiments, remote viewing, and Schmidt-type of research.

What this history reveals is that the parapsychologists at any time have a pool of candidates for the iron-clad, repeatable experiment to put before the critics. The problem is that the members of this pool keep shifting in and out. Yesterday it was sheep - goats, Levy's experiments with implanted electrodes, and dream telepathy. Today these experiments are hardly mentioned by proponents and are rarely carried out. By analogy, then, we have to be cautious in taking too seriously the current contenders. Experience shows that the most promising research programs in parapsychology will most likely be passé within a generation or two.

But I also pointed out more specific reasons for hesitating to accept Schmidt's results. Some of these reasons overlap with those Hansel has given. Schmidt places complete reliance on his machine to protect the integrity of the experiment. The subjects, for the most part, are unsupervised and unobserved. The assumption is that instrumentation can replace old-fashioned controls in human experimentation. This assumption is unacceptable for a variety of reasons. An obvious one is that, no matter how sophisticated and automated a random-number generator may be, we still must learn its properties by a lot of experience and testing. Rarely does any new device behave fully according to its theoretical or expected specifications. It takes time to discover the biases and peculiar properties of the new gadget. And this difficulty seems to increase, rather than decrease, with the sophistication and complexity of the device.

Ironically, Schmidt keeps changing the design and components of his random-number generator from experiment to experiment. This has the desirable property of achieving generality among devices — *if* one could show consistency of results. But, at the same time, it prevents us from gathering the cumulative experience with one particular generator to fully understand its peculiarities and to properly "debug" it.

Neither Hansel nor I was fully satisfied with the control trials employed by Schmidt. Schmidt allows the machine to run during periods when subjects are not trying to influence its output, and he makes sure to run it on such control series before, during, and after the experimental

series. In principle this is highly commendable. If the machine has any long-term, or even temporary, trends away from equality of outputs this sort of control might catch it. But Schmidt does not conduct these control series systematically, and Hansel's suggestion is important. Hansel feels that control and experimental runs should be conducted in pairs and that which particular run will be a control or an experiment should be decided by a randomization procedure. The experimenter monitoring the generator should be kept blind as to whether the run is a control or not.

Hansel probably sees this control as another constraint upon trickery. But I see it as a necessary control against possible short-run biases in the generator output. Schmidt's subjects try to affect the generator output only for runs that last for relatively short bursts. The control runs, however, cover extended intervals that are many orders of magnitude in length compared with experimental runs. If the generator has short-run biases, these could easily fail to be detected by the sort of tests that Schmidt applies to the control runs.

I could continue listing further possible weaknesses in Schmidt's experiments. These weaknesses are of two kinds. There are general weaknesses that one would point out as flaws in any experiment involving human subjects and information transfer. The fact that subjects are neither systematically observed nor treated in a uniform way, for example, would be such a weakness. Other weaknesses would be specific to this particular sort of paradigm and its objectives. For example, the fact that Schmidt's tests for randomicity only test dependencies once removed and not beyond is such a weakness.

Of course no single experiment can conceivably control or systematically deal with every relevant variable. Indeed, the point of doing research is to discover those conditions and variables that *are* relevant. When we point to a procedure or the omission of a procedure as a "flaw" in the design, we are making an informed judgment. We are saying that such a procedure or precaution was both reasonable and feasible under the given conditions. Further, we are stating our belief that a competent investigator would have taken the matter under consideration.

Pointing to some flaws, for example, is equivalent to suggesting alternative reasons for the outcome. If Schmidt had not frequently tested his generator for bias, then pointing out this oversight would be equivalent to strongly suggesting that the alleged PK results were simply the result of a systematic bias in the machine. But, of course, Schmidt did conduct such tests. Both Hansel and I consider the way he carried them out to be a weakness in the studies. But this latter type of weakness, while an obvious departure from the ideal, does not automatically provide an alternative explanation for how Schmidt obtained his results.

Hansel, as a critic, feels called upon to provide alternative explanations for the results. He restricts his search for alternatives completely to deliberate trickery on the part of the experimenter, the subject, or an outsider. His position is that, if he can conjure up a scenario in which trickery *could* have produced the results, then the resulting experiment cannot provide evidence for psi.

The parapsychologists, of course, see Hansel's position for what it is — a dogmatism that is immune to falsification. There is no such thing as an experiment immune from trickery. Even if one assembles all the world's magicians and scientists and puts them to the task of designing a fraud-proof experiment, it cannot be done. I could always insist that, of the infinite number of variables not explicitly taken into account in this "fraud-proof" design, many of them — ones still unknown to us — could leave loopholes for a form of trickery we have not yet discovered. In practice, it would be impossible even to take into consideration all the known variables that could allow some form of deception.

I think it is possible and rational for skeptics to avoid committing themselves to this false dichotomy — that the results must be either paranormal or fraudulent. There are other alternatives, many of which we have yet to learn about, but I do not think it necessary or wise to feel that we must always provide an alternative explanation for alleged paranormal claims.

Applying these considerations to Schmidt's work, I think the wise course is to wait. The work is in its preliminary stages. The generators have been neither standardized nor debugged. The research paradigm is still fluid and far from scientific. The results are provocative but far from lawful, systematic, or independently replicable. We have no need to try to explain or account for any of this now. Only when the parapsychologists settle upon a standardized paradigm, tidy up the procedures, demonstrate that the results follow certain laws under specified conditions, and that these results can be duplicated in independent laboratories, will we have something that needs "explaining." Of course by the time circumstances reach such an orderly stage there may very well be nothing left to explain. So far, in my opinion, this has been the normal course of events in things parapsychological.

The drawback of my position is that it counsels patience. We might never know, by following my advice, just what did account for Schmidt's data. But the alternative, which is to insist on settling the matter now, leads to the inevitable shrill claims, on the one side, that here is proof of psi and, on the other side, that cheating must be going on.

One more aspect of Hansel's approach bothers me. Each experiment, he says, must stand on its own feet. (Such a demand, unfortunately, is at

odds with every contemporary history and philosophy of science account that I have read.) In "analyzing an experiment it is wise *initially* to adopt the assumption that ESP (or in this case PK) is impossible and then see how the result could have arisen through already established processes." If Hansel had worded this somewhat differently, I think I could agree. If he had said, for example, that it is wise to assume that the initial odds in favor of psi are exceedingly low, I could not take exception; for Bayesian and other models of rational behavior still allow for some change in these odds as a result of new empirical data. But, if we start with the initial assumption that the odds in favor are zero, then no amount of empirical evidence can change our position.

Notice that a feature of such models of rational judgment hinges crucially upon how we conceptualize the outcome. Hansel's critique reads as if he had restricted the outcome to just two possibilities — psi or fraud. But even these two possibilities may not be simple or mutually exclusive, and they certainly far from exhaust all the possibilities. The category "psi," even among parapsychologists, covers a number of existential possibilities. Some talk about a category of phenomena that are independent of any physical laws now known or conceivable. Others see new types of phenomena and forces that were hitherto unknown but entirely compatible with modern physics. We even find some parapsychologists arguing that, when properly understood, psi phenomena result from the operation of already known forces, such as extremely low frequency waves. And fraud ranges from deliberate, conscious cheating to various psychological aberrations and self-delusions. But in between these complex alternatives is a vast array of other alternatives involving the operation of statistics of rare events, subtle subject-experimenter-environment interactions, improper but nondeliberate manipulation of data, and many, many other possibilities. Among these alternatives could very well be new sorts of biases or ways for experiments to go wrong that we don't know about. In my own field of experimental psychology we still are uncovering novel ways in which experiments can be biased. There is certainly no reason to suspect that all the known ways that experiments in parapsychology can go wrong have been discovered.

In short, I see no need to rush matters. I agree with Hansel that the data so far produced by parapsychologists do not justify the claims for the existence of something called "psi." But I see no need to buttress such a conclusion by creating scenarios in which trickery could have occurred. Why try to account for something that does not yet need accounting for? •

Helmut Schmidt offers this brief reply to the two preceding articles:

I planned a detailed reply, but doubt whether that would serve any valuable purpose: It would certainly not be appropriate to argue with Hansel about my honesty. I am also certain that he would not be any happier if I were to assure him that the subjects were closely supervised so that they could not tap the signal line between random generator and recorder and if I were to respond to some other technical points. (For example, the careful reader of my paper [Hansel's note 4] learns that the random generator "can produce sequences of binary random numbers of any specified length." Nevertheless, Hansel suggests that the random generator contains no means for stopping at the end of a run.)

Ray Hyman's mixed feelings about modern parapsychology, particularly in view of the past history of the field, are quite understandable. If I did not happen to work actively in the field but had to rely on the reports of other workers, I might have quite similar feelings. Hyman's technical comments reflect differences in taste, I think, rather than major issues. Many more randomness tests were done than published to satisfy my own questions about the possibility of temporary random generator malfunctions.

—Helmut Schmidt
Mind Science Foundation
San Antonio, Texas

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Atlantean Road: The Bimini Beach-Rock

Charles Berlitz and other writers have rhapsodized about the discovery of Atlantean ruins off the coast of Bimini in the Caribbean. These "ruins" consist of "roads" that run under the sea parallel to the shore. Marine archaeologist J. Manson Valentine and historian David D. Zink, funded by the Edgar Cayce Foundation, came to the conclusion that these roads were the "real goods."

However, geologists John Gifford and Eugene A. Shinn have easily proved (see *Sea Frontiers*, May-June 1978) that the Bimini Road of Atlantis is merely an example of what is called "beach rock." (See also the article by archaeologists Marshall McKusick and Shinn in *Nature* 287 (1980): 11-12.) Beach rock is a rapidly formed conglomerate of sand grains cemented together by calcium carbonate in the sea to form slabs that break into strips when undercut by tidal action; these then can present the appearance of large "bricks" of stone laid together.

In researching this subject, I found that beach rock was found on the coast of Australia. Visiting there recently at the invitation of Dick Smith, I was flown along the coast in a helicopter and saw enormous masses of this material along the shore north of Sydney. Examples of photos I took on that occasion are shown in Figures 1-3.

Do Berlitz, Valentine, and Zink ask us to believe that Atlantis ran a branch office in Australia? Or did the Atlanteans reinforce the coastline of the Australian continent as an engineering experiment?

— *James Randi*

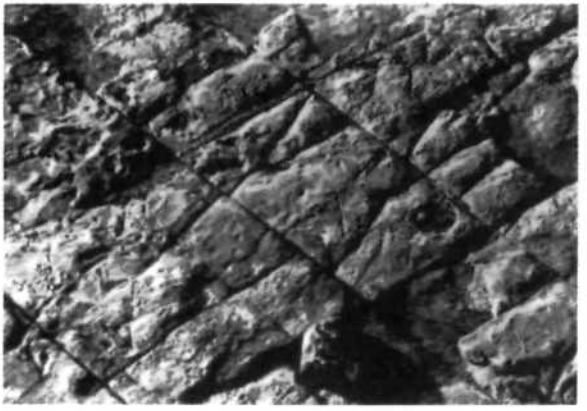

FIGURE 1. "Young" beach rock presents this appearance. The first fractures are those passing from upper left to lower right. Secondary fractures occur afterwards, thus more or less randomly and not as regularly as the primary ones. The appearance of man-made "bricks" is quite strong.

FIGURE 2. Older beach rock is weathered so that cracks have eroded. The constant action of water-plus-sand abrasion sloshing back and forth cuts rounded edges — and further accretion of sand grains gives the impression of "mortar" between the individual rocks. As for scale, the round dark hole at top center is about ten feet across.

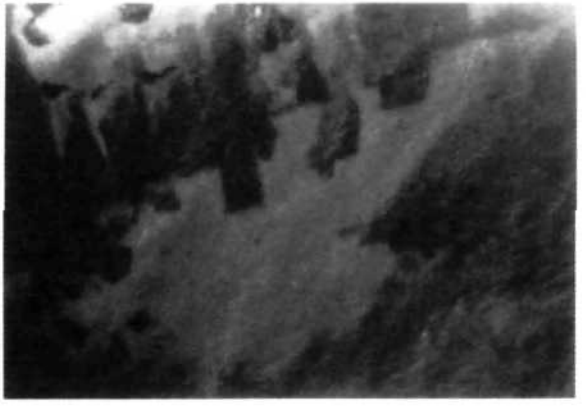

FIGURE 3. Seen underwater, beach rock formations look — to the uninformed — *very much like ruins left* behind by man.

Deciphering Ancient America

Barry Fell's claims of ancient New World settlers fall before scholarly analysis

Marshall McKusick

A retired professor of marine biology from Harvard, Barry Fell, attracted an immense lay following with his best-selling book, *America B.C.: Ancient Settlers in the New World* (1976). The book created a sensation because the author combined Harvard credentials with very unorthodox conclusions: he claimed that pre-Columbian America had been colonized by Celts, Phoenicians, Egyptians, Libyans, and other ancient peoples. Its commercial success may be judged by the fact that the American Booksellers Association presented it to the White House library as one of the 250 best books published in the United States during 1973-1977.

Has the reading public both here and abroad been victimized by archaeological fiction posing as legitimate research? Some professional archaeologists have claimed that Fell is a deluded scholar whose statements represent compounded errors. Nevertheless, *America B.C.* obviously offered the reading public something it wanted, perhaps the hope that American prehistory was an ethnic record of European and Mediterranean ancestors. Others enjoyed the vigorous style with which Fell attacked traditional archaeological research. The conflict between the professionals and Fell has continued with the publication of his most recent book, *Saga America* (1980), which introduced Vikings and other adventuresome peoples as an explanation for the past [see John R. Cole's review in the Fall 1980 *SI*].

In the midst of claims and counterclaims Fell has been able to convince a large number of followers that American prehistory is a record of Old World civilization. Among the claims that Fell makes are:

1. Egypto-Libyans left stone memorials, such as the Long Island

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Marshall McKusick, professor of anthropology at the University of Iowa, Iowa City, has published extensively on the question of Old World contacts with pre-Columbian America.

Tablet and the Davenport Tablet from Iowa, or had been the source of the pictographic writing on these stones, taught the Canadian Micmac pictographic writing, left written messages in the greater Southwest and elsewhere, and explored Polynesia. Later, the Greeks and Libyans mingled and were responsible for the Greco-Libyan place names found in New England. To assist in overseas Pacific voyages, Libyan-Americans establisted a naval academy in Nevada and schools in Colorado and California, and they mapped Hawaii.

2. Celto-Phoenicians established an international fur trade between the hemispheres, shipping lumber, silver, copper, gold, furs, and other commodities to Europe and the Mediterranean in exchange for manufactured goods. The Celts of Spain taught the Phoenicians an Irish form of ogham alphabet, which was used for American inscriptions written in both Celtic and Phoenician. Iberian Celts established commercial banks in the west.

3. Other inscriptions and coins found in America demonstrate that many nations influenced pre-Columbian America, including Greeks, Romans, Jews, Islamic Arabs, North African Christians, and Byzantines.

4. Scandinavian Vikings, later joined by Irish and other Celts, traveled extensively in North America by circumnavigating Florida and exploring the Mississippi and western tributaries. The Norse left written inscriptions in the Southwest, Oklahoma, and elsewhere, as well as building Newport Tower in Rhode Island.

Scripts and Languages

Successful decipherment of a text depends upon the correct identification of the alphabet or the other characters used. Where these are phonetic in value, as in alphabetic or syllabic scripts, the pronunciation of the word identifies the language. While this is a simplification of the complexities involved in a true decipherment of a previously unread script, it does represent a fundamental necessity. If the script is incorrectly identified, there is no chance of a successful decipherment. On these grounds the attacks by scholars on Fell's identifications of scripts lie at the heart of their rejection of his work. Scholarly challenges have been made of Fell's identification of ogham, Egyptian, Libyan, Punic, runic, and petroglyphic characters.

Fell contends that ogham (or *ogam)* script was used in New England and elsewhere in North America by ancient Celts and, in addition, that the Phoenicians learned to use it from these Celts, writing their Punic language without vowels. However, two British archaeologists familiar with Celtic ogham have demonstrated that these scripts have not been correctly

identified in America (Ross and Reynolds 1978). Irish ogham developed after the fourth century as a system using groups of linear marks to represent 15 consonants and groups of dots for 5 vowels. Although this script is innovative and separate from traditional European alphabetic characters, the phonetic values of the signs are derived from written Latin. The distribution of ogham was restricted to one part of Ireland and adjacent colonies in Wales and Scotland and was largely limited to writing funerary inscriptions on stone. Because of its Latinized origin, it is impossible for ogham to have been known and used by Spanish Phoenicians or American Phoenicians 1,000 years before it was invented in Ireland. The two British archaeologists visited Vermont where the alleged ancient ogham was reported. They found that some of these marks were of comparatively recent origin. At the Crow site in Vermont, Ross and Reynolds (1978) positively identified the marks; they were not ancient writing but were marks left by some Yankee farmer who ran his plow across a field boulder. The archaeologists identified the plow as a single share, the Gloustershire type, and the plow marks paralleled the stone fence row. Other so-called ogham marks were identified as erosional grooves and natural striations. At the New Hampshire site named Mystery Hill, Fell's reported appearance of a Roman numeral "thirty" on the Beltane Stone was alleged to be part of a Celtic calendar. More recently Cazeau and Scott (1979) demonstrated that some of the markings on this stone were caused by granite veins that were misidentified as an intentional inscription.

Fell's ogham hypothesis proved to be very attractive to the unwary antiquarians who reported linear markings in New England and throughout North America. As one example, Fell read three parallel lines as ogham script "BL" and, supposing it to be a Phoenician word, added the dipthong "AA," which he then identified as a dedication to the Canaanite god Baal. Another instance of self-deception is Fell's decipherment of some lines on the Bourne Stone from Masachusetts, marks allegedly "Phoenician-ogham" and translated as a report of the annexation of the territory into the Carthaginian empire by General Hanno—literally: "A Proclamation of annexation. Do not deface. By this Hanno takes possession" (Fell 1976, p. 160). No Phoenicians are known to have been west of Morocco, and no valid evidence exists for the speculation that they reached Massachusetts or that they wrote in an Irish script.

Where Phoenician-ogham cannot be made to serve the purposes of Fell's flights of fancy, other linguistic inventions are forthcoming. One of these is a rendering of the Grave Creek stone as the Phoenician "Punic" language written in "Iberic script." According to Fell (1976, p. 158), this small tablet was found in 1838 in a stream bed in central West Virginia and

reads: "The memorial of Teth. This tile (His) brother caused to be made." Because Fell either is ignorant of the relevant archaeological literature or rejects it without proper citations, a lay reader will be deceived by this interpretation. The Grave Creek tablet was thoroughly discredited as a hoax perpetrated to add an attraction to the public museum associated with the 1830s excavations of the Grave Creek mounds. Testimony from the workmen, obtained in later years, showed this artifact had no clear provenience; and the short inscription actually represents an assortment of letters from different alphabets. All of this information has been available since the evaluation by Thomas (1894).

Scripts that Fell identified as Libyan, hieroglyphic Egyptian, and hieratic Egyptian have fared no better than his decipherments of plow-mark ogham. The Davenport Tablet, which Fell identifies as a trilingual inscription in Egyptian, Libyan, and Punic, is thoroughly documented as a hoax made by members of the Davenport Academy of Science in the fall of 1876. This hoax became part of a broader conspiracy to deceive a local German clergyman who irritated Yankee academy members by his mound-digging activities. In the 1880s Smithsonian staff members exposed various Davenport frauds, and this affair became more thoroughly documented in the 1960s when confessions and other written testimony appeared (McKusick 1979a). Fell still defends his theory of Egyptians in Iowa (1980, p. 109), although Jonas Greenfield, professor of Semitic languages at Hebrew University, informs me that the scripts are not Egyptian, Punic, or Libyan. The Davenport Tablet is marked up with obvious Greek letters, musical cleff signs, ampersands, and a mixture of letters never associated with each other in the Old World. Cyrus Thomas, one of the Smithsonian archaeologists of the 1880s, suggested that the forgers copied this heterogeneous collection from the alphabets illustrated in *Webster's Unabridged Dictionary,* 1872 edition.

The Long Island Tablet found in the 1880s is a bilingual record written, according to Fell (1976, p. 270), in Egyptian and Libyan characters. Anthropologists, beginning with Brinton in 1893, identify the marks as typical aboriginal petroglyphs, including a bow and arrow, a man and a canoe, a possible wigwam, and those resembling a bird, a deer, a fish, an eel, and a bear's paw. No mention or citation of the anthropological interpretation is made by Fell, who transformed the canoe into a Libyan galley and translated the inscription to read: "This ship is a vessel from the Egyptian Dominions."

The transformation of aboriginal petroglyphs into Old World scripts occurs throughout *Saga America,* where crudely redrawn sketches from standard anthropological studies are variously labeled and deciphered as Arabic, Libyan, Chinese, and even Scandinavian. These anthropological

studies were based upon petroglyphs from California and adjacent states in the Great Basin and Southwest, and even from British Columbia, locations that lead Fell to speculate about far-flung ancient conquests in western North America by Celts, Vikings, and various Mediterranean nations. No anthropologist familiar with aboriginal petroglyphs accepts any of Fell's identifications.

Runic letters of Scandinavia continued to be used for short inscriptions long after the more efficient and complete Latin alphabet came into use in medieval times. While some runic inscriptions have been found in the Greenland Norse settlements, and farther north, where one was found on a rock cairn, not one genuine runestone has been reported from North America. For years specialists in medieval Scandinavian studies have written about the famous American runic forgeries, such as the Kensington and Spirit Pond stones, and the misidentified "runes" from Oklahoma and elsewhere (Wallace 1971). Fell, in *Saga America,* accepts various types of false evidence as genuine. Some of these so-called runes are simply not Scandinavian letters, and among examples one can point to the misidentification of markings on Newport Tower, which Fell and others claim to be Scandinavian. Another group of errors is the identification of pictographs as Norse-inspired pictorial art, although no such drawings are found in Scandinavia. Finally, Fell has reported the Pelham Stone from New Hamphsire to be an attempt by a Norse trader to contact the Indians by means of petroglyphs, a supposition unsupported by any known evidence. With such inventions, it is hardly surprising that he reports Welsh-ogham from Oklahoma and traces joint ventures in America by Celts and Norsemen.

Languages from the Scripts

Specialists in Amerindian languages have failed to find any trace of Old World grammar or vocabulary having a pre-Columbian origin (Goddard and Fitzhugh 1978). However, Fell ignores such conclusions and projects major contacts expressed in language relationships. Among the linkages he reports are so-called "interface tongues," which are represented as Old World and New World mixtures exemplified by Libyan-Zuni, Iberic-Pima, Basque and Celtic-Algonkian, and Greco-Libyan-Algonkian. Micmac-Algonkian "hieroglyphic writing" is said to represent the adoption of a writing style rather than a language transferral.

Since the scripts have been misidentified by Fell, much of his discussion about Celtic, Welsh, Iberic-Punic, and other languages is illusionary. Nevertheless, not all of his conclusions can be rejected on these grounds alone. Not all of his claims deal strictly with phonetic scripts from abroad

— for example, he identifies the modern spelling of New England place names as concealing more ancient Celtic root words. In this case, a Celtic specialist has shown that not one of the numerous examples mentioned by Fell represents a genuine pre-Columbian loan word of Celtic origin (Nicolaisen 1979). There is an aura of uninformed speculation surrounding such linkages as Libyan-Zuni and Libyan-Polynesian, but so far no professional linguist has published an analysis of such claims. Since the grammar and morphology of these three languages reflect totally different origins from the perspective of historical linguistics, the comparison must be considered fallacious, pending the application of orthodox linguistic methodology. The task may not be easy in one sense, because a linguist will have a great deal of difficulty with the "Libyan" part of the equation. Fell has scrambled together various scripts from North Africa, invented phonetics and vocabulary, and ignored grammar and has seemingly constructed the Libyan language without reference to known linguistic relationips from the Mediterranean area. It appears that "Libyan," which he traces to locations throughout the world, is an illusionary and phantom language.

The Archaeology of Deception

The history of successful decipherments is littered with footnotes about unsuccessful attempts that for a time seemed somewhat plausible. From eighteenth century onward there were many attempts made to "read" Egyptian hieroglyphics and other scripts, and these efforts by well-intended antiquarians were frequently published. Fell's numerous claims represent a culmination of amateurish speculations. He has ventured into American prehistory and attempted to rewrite the past by ignoring professional archaeological studies. His evidence has rested upon phony linguistics. However, in contrast to Fell's claims of numerous foreign scripts, archaeologists have yet to accept a single valid case of a pre-Columbian inscription in the New World (McKusick 1979b). Fell's well-illustrated books appear to present archaeological evidence, until one recognizes that most of the artifacts either are aboriginal petroglyphs or have some other explanation, such as plow-marked ogham, natural striations, or misidentified lines of recent origin.

It may be added that two other lines of inquiry also appear in his writings. Various Old World coins have been found in the New World, which he accepts as supporting proof of his claims of massive pre-Columbian trade and cultural contacts. A study of this coin evidence shows that some of the coins, such as "Hebrew shekels," were recently minted souvenirs, and not one other example has a solid archaeological

context suggesting antiquity (Epstein 1980). Furthermore, the archaeological structures illustrated and described by Fell, such as those from New England, are not Celtic and Phoenician shrines. Those that have been studied in Vermont have a known Yankee origin and include stone cellars once put to a variety of uses, such as storing turnips for the sheep industry (Neudorfer 1979). Supporting excavations have been made in Massachusetts, where stone structures of various kinds are related to former Yankee settlements (Cole 1980). Because the Mystery Hill site in New Hampshire was extensively disturbed and rebuilt by a former owner, some of the structures are now difficult to identify; but there is nothing that suggests either a Phoenician or a Celtic origin and, in particular, Fell's "sacrificial altar" is a Yankee lye stone (Swauger 1980).

The challenges presented by Fell and his imitators are difficult to meet because traditional linguistics and archaeology lack the drama of the strange, mysterious, and the unexplained, which is the stock-in-trade of the popularizers. Until a greater effort is made to reach the public with more factual accounts of prehistory, the mythology about lost races and ancient nations will persist as a substitute for scholarship.

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A Sense of the Ridiculous

Where is it when we need it?

John A. Lord

In studies of the paranormal, we are frequently urged to maintain a due skepticism. I say "due," because, of course, a *total* skepticism in the face of overwhelming evidence is unscientific and self-defeating. But the Committee for the Scientific Investigation of Claims of the Paranormal insists that paranormal claims not be rejected a priori, antecedent to inquiry. I want to suggest that this position is too liberal, because it places the onus of proof on the skeptic.

Part of the problem for most of us is that proper inquiry usually takes up a great deal of time, often costs money, requires considerable specialized knowledge and expertise, and may well induce a distinct feeling of sterility and wasted effort, especially when that effort could have been devoted to more obviously profitable studies. (Carl Sagan, for example, comments ruefully that the writing of his critique of Velikovsky diverted him from his mainstream research.[1])

I am sure, though, that the scientists and magicians who carry out such work would justify it in two ways:

1. It fulfills a useful function by educating an apparently incredibly credulous public, showing them the errors to which so much paranormal research is prone (and at the same time countering the popular accusation that scientists are by nature aloof toward and

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biased against the "new paradigm").

2. It gives the critical investigator an opportunity to be involved in some possible (though rather improbable) major developments in our concepts of man and the universe. Marks and Kammann mention this a few times in the course of their recent book,[2] and I am quite sure that James Randi would not begrudge a single penny of his $10,000 prize if, by awarding it, he could be the first to publish a cast-iron, replicable paranormal finding in the columns of, say, *Nature:* certainly it would guarantee him immortality in the history of science.

But what of the rest of us? How can those skeptics who are (like myself) neither trained scientists nor professional magicians hope to assess the claims that are constantly being made? Do we have a weapon in our armory that we can use to combat at least *some* of the wild claims that are made, not only by unscrupulous journalists but also by "hard-headed" scientists who work in laboratories?

I suggest that such a weapon is available: I suggest that most (if not all) skeptics possess a quality that is singularly lacking in many paranormalists and their followers. It is not simply that we are rather more, and they rather less, skeptical or critical. It is rather that we possess a sense of the *ridiculous,* whereas they do not. Anyone having this quality is less likely to be taken in by insubstantial claims. Of course it is not sufficient in all cases: there will always be some claims that will demand minute critical scrutiny, but it does at least rule out, at a stroke, the grosser manifestations of the paranormalists.

By "a sense of the ridiculous," I do not simply mean a sense of humor: too many critical writers have an unfortunate habit of lapsing into facetiousness when discussing the evidence for the paranormal, making feeble jokes at the expense of "ideas" that deserve more robust treatment.

Now it is true, as I have said, that many paranormal claims are not, on the face of it, a priori ridiculous: biorhythms would be a good example. It is an undisputed fact that certain organisms display periodicity, and given the existence of, say, the human menstrual cycle and of some circadian rhythms, it would be foolish (without further investigation) to laugh at the proposition that there are 23/28/33-day cycles in all human beings. Of course the suggestion that such cycles are constant and unchanging has a suspicious ring: it sounds altogether *too* procrustean. But the claim is not so ludicrous that it might not be true, and so it demands (and has been given) proper scientific investigation.[3] Much the same might be said of the Gauquelins' claims for astrobiology.

What I have in mind when I speak of using a sense of the ridiculous are those cases that are so patently silly that to give them serious attention

presumes a worth in them that they clearly do not possess. They are so manifestly bankrupt that to give them critical consideration would be to insult the readers of such criticism—and it would insult them by assuming that, among other things, they did not have a proper sense of the ridiculous.

Take Uri Geller, for example. I never saw his performances on British television in the early seventies, but I subsequently gathered from the newspapers that something very peculiar had taken place. Did I believe it all? (I was fairly credulous at the time.) I can't actually recall caring very much one way or the other. Of course Geller was subsequently exposed by various investigators; but before I ever read their versions of what had probably taken place, I came across a statement made by Dr. Puharich, Geller's manager. He said that Uri derived his powers from computers stowed in flying saucers from the planet Hoova! Further investigation was now unnecessary. Uri was bending metal by either normal or paranormal means: if normal, then it was a conjuring trick; if paranormal, then it was due to some distinct and measurable physical forces and not, emphatically not, to psychic vibrations from "Hoova" or anywhere else.

Another good case is that of Harry Price, the doyen of British ghost-hunters, who founded his own "National Laboratory of Psychical Research" and managed to create the impression that he was a critical, skeptical practitioner. His greatest triumph was the investigation of Borley Rectory (the "most haunted house in England"), about which he published two best-selling books. The claims he made were backed by eloquent testimony from responsible and respectable people, and for many years it seemed as if Price had succeeded in producing a genuine record of well-authenticated phenomena. There was a definite case to answer, and it was answered decisively in two books that appeared after his death.[4] From these thorough, soundly researched books, Price emerges as a rogue and a charlatan, a bare-faced liar and a falsifier of evidence.

The story badly needed telling, and the authors tell it superbly. But the real problem is, should it have needed telling? The public was taken in by Price's Borley books, and massively taken in at that. But should people have been taken in at all? Not if they had any sense of the ridiculous, coupled with a knowledge of Price's other activities. For at the time he was engaged on his Borley investigation, Price published (in collaboration with R. S. Lambert) a book called *The Haunting of Cashen's Gap.*[5]

This book purported to be a serious inquiry into the case of Gef, an 84-year-old talking mongoose from the Isle of Man. I should point out that, not only have I never read this book, but I have no desire to do so (there are, I think, better things I can do with my time). Now if Price were (as he claimed to be) a serious critical investigator, how could he bring himself to

waste his talents on such palpable twaddle? And, if he didn't actually believe it for a moment, why take up the public's time with this nonsense? On the first assumption, Price would be reckoned a fool; on the second, a knave, and clearly not worth further consideration by any sane person with a sense of the ridiculous.

Turning our attention from talking mongooses to talking horses, we encounter Lady, who was investigated (and found genuine) by no less a pioneer than J. B. Rhine.[6] So far, I am unimpressed, if slightly amused. Of course apologists might urge that this "research" was carried out early in Rhine's career—are we not all entitled to a few mistakes? On the other hand, we are always reminded that, when Rhine embarked upon his career as a parapsychologist, he was already a trained scientist (a botanist, in fact). In any case, Rhine continued to have no doubts about Lady. Nearly thirty years later, he wrote, with sublime innocence, "Experimental work . . . has already produced evidence of good exploratory character that at least one horse . . . has shown behavior consistent only with the psi hypothesis."[7]

Rhine's *major* work has now been placed in a far less positive light by Hansel;[8] but even so, we must ask how anyone could be deceived by such an obvious music-hall act as a talking horse. It is not simply that Rhine must have been an incompetent investigator, as Milbourne Christopher hints.[9] Anyone who is taken in by such ridiculous pantomime games is clearly lacking a sense of the ridiculous, and how such a person could ever come to be regarded as a leader in his chosen field is beyond my comprehension.

Examples of such nonsense could be multiplied ad nauseam. I shall add just one more. When Betty Markwick finally demolished the already shaky experiments of S. G. Soal,[10] J. G. Pratt made a reply. Markwick pointed out that the targets used by Soal were *not* compiled from random number tables as had been claimed. Instead, Soal, either consciously or unconsciously, had prepared targets that were anything but random. In his rejoinder, Pratt states:

> I do not mind revealing that I am the person who suggested that Soal might have become his own subject on some occasions when preparing the list of random numbers on the record sheets before the sittings were held. This "explanation" [my scare quotes] would require that he used precognition when inserting digits into the columns of numbers he was copying down, unconsciously choosing numbers that would score hits on the calls the subject would make later. For me, this "experimenter psi" explanation makes more sense, psychologically, than saying that Soal consciously falsified his own records, but I do not argue that it should be accepted by others as the likely interpretation.[11]

Faced with such willful credulity as this, what can one possibly say? The case is obviously more subtle than the others I have quoted, for Pratt is trespassing against more than our sense of the ridiculous—he is betraying his fundamental ignorance of a significant principle of the philosophy of science: ad hoc shifts should play no part in serious scientific discourse. If our theory does not account for observed events, we salvage what we can and abandon the rest. We do *not* engage in attempts to patch it up and claim that it still works but in a different way. I can only agree with E. J. Dingwall when he says that "perhaps Andrew Lang . . . was right, when he was reported as saying that sometimes it looked as if psychical research does somehow change and pervert the logical faculty of scientific minds."[12]

In the face of Pratt's folly, I can only laugh: are not, after all, such reckless attempts to save a doomed experiment totally ridiculous?

But I do not expect that this will be appreciated by the believers, for who cares to be laughed at? Am I not myself, they may ask, being ridiculous: over-*in*credulous, and unscientific into the bargain? I can only reply that a critical treatment of claims as absurd as those I have instanced would be a totally inappropriate response. If someone wants to tell me that dice experiments show that some people are luckier than others, or that card-guessing experiments demonstrate that some people are better at guessing than they ought to be, then that is fine: at least it gives us something solid to bite on, something that might be worthy of critical investigation. But flying saucers from Hoova and talking mongooses? No, the only proper response to such ridiculous claims is to laugh them out of court. Serious argument would be a waste of time not only for the skeptic but (dare I say it?) for any sane person.

Notes

1. D. Goldsmith, ed., *Scientists Confront Velikovsky* (Ithaca: Cornell University Press, 1977), p. 93; C. Sagan, *Broca's Brain* (New York: Random House, 1979), p. 127.

2. D. Marks and R. Kammann, *The Psychology of the Psychic* (Buffalo, N.Y.: Prometheus, 1980), pp. 5, 44, 76.

3. See T. M. Hines, "Biorhythm Theory: A Critical Review," SKEPTICAL INQUIRER, Summer 1979, pp. 26-36.

4. E. J. Dingwall, K. M. Goldney, and T. H. Hall, *The Haunting of Borley Rectory: A Critical Survey of the Evidence* (London: Duckworth, 1956); T. H. Hall, *Search for Harry Price* (London: Duckworth, 1978).

5. H. Price and R. S. Lambert, *The Haunting of Cashen's Gap* (London: Methuen, 1936).

6. J. B. Rhine and L. E. Rhine, "An Investigation of a 'Mind-Reading' Horse," *Journal of Abnormal and Social Psychology* 23 (1929): 449-466.

7. J. B. Rhine and J. G. Pratt, *Parapsychology: Frontier Science of the Mind* (Springfield: C. C. Thomas, 1957), p. 85. This book was revised in 1962. If Rhine had any reservations by then, he did not voice them.

8. C. E. M. Hansel, *ESP: A Scientific Evaluation* (New York: Scribner, 1966). Subsequently enlarged and revised as *ESP and Parapsychology: A Critical Re-evaluation* (Buffalo, N.Y.: Prometheus, 1980).

9. M. Christopher, *ESP, Seers and Psychics* (New York: Crowell, 1970), p. 45. The British edition, published by Cassell in 1971 has the title *Seers, Psychics and ESP.*

10. *Proceedings of the Society for Psychical Research* [of Great Britain] 56 (May 1978):211.

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12. E. J. Dingwall, "Responsibility in Parapsychology," in *A Century of Psychical Research: The Continuing Doubts and Affirmations*, ed A. Angoff and B. Shapin, Proceedings of an international conference, Le Piol, St. Paul de Vence, France, Sept. 1970 (New York: Parapsychology Foundation, 1971), p. 49. ●

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Book Reviews

The Encyclopedia of UFOs. Edited by Ronald D. Story. Doubleday, Garden City, N.Y., 1980. 440 pp. $12.95, paperback.

Reviewed by Daniel Cohen

As a general rule it is a bad idea for a book to be reviewed by one of its contributors, but my own contributions to *The Encyclopedia of UFOs* are so minor that in this case I think the rule can be ignored. In fact, my participation in the project gave me the advantage of some insight into how this book was put together.

Back in October of 1977, I was asked by Ron Story to suggest some articles for the planned *Encyclopedia of UFOs* and to prepare a brief autobiography and a statement of my position on UFOs. I wrote one article, a bio, and a position statement. Last year, after a long and, I assume, tortuous process *The Encyclopedia of UFOs* finally appeared. Obviously a large number of people connected with UFOs responded to Story's request as I did. A notable holdout was Phil Klass, who declined to have anything to do with the project.

The result of all of this is a curious sort of book — actually it is two books. The first is a collection of articles on various UFOlogical topics; the second is a collection of biographies and position statements from all sorts of people who have had something or other to do with UFOs. The statements range from the short and petulant (my own) to the long and thoughtful (Thornton Page's and Robert Sheaffer's) to the absolutely nutsy (Frank Stranges's). If an individual did not provide a position statement, the editors put one together from his or her previous writings and speeches.

Since UFO pictures are hard to come by, the majority of pictures in *The Encyclopedia of UFOs* are portraits of the various "leaders in the field." On flipping through the book my first impression was, "My God, we are a scruffy-looking lot."

Oddly, there is no photograph, biography, or position statement from editor Story himself, though his book *The Space Gods Revealed* is generally considered one of the best on the von Däniken nonsense, and a Story article on von Däniken

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Daniel Cohen is the author of many books about fringe-science claims. He has written often on UFO reports.

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appeared in the Fall/Winter 1977 issue of the SKEPTICAL INQUIRER (then the *Zetetic*). Since Story not only edited the *Encyclopedia* but also wrote many of the articles in it, a little background would have been useful. There is a biography and a position statement from J. Richard Greenwell, who is listed as consulting editor, and, though I did not count, it appears that Story and Greenwell wrote more of the articles for the *Encyclopedia* than did any of the other contributors.

I cannot imagine that the biographical part of this book will be of much interest or value to the general reader, or to anyone who did not actually participate in the project. Of course I enjoyed attaching faces to names that I had known only through correspondence or third parties and seeing how much older some of my friends and enemies had grown. I also had some fun finding out what this or that obscure individual thought about UFOs back in 1977 or 1978. But how many people not already deeply involved in UFOs will care? How many should? This part of *The Encyclopedia of UFOs* struck me as a space-filler.

The second part of the *Encyclopedia* (which I would estimate makes up two-thirds of the book) is the articles themselves. As in all encyclopedias where the articles are provided by a mixture of different authors and "experts," these are a mixed bag, a good deal more mixed than in a standard encyclopedia. The authors represent all shades of UFO belief and nonbelief, and they approach their subjects from widely different points of view and with varying degrees of competence and writing skill.

Often in a work of this type a strong editorial hand is applied, but editor Story seems to have declined to do this. As a writer I'm entirely sensible of the advantages of editorial nonintervention, but laissez faire has its disadvantages too. An encyclopedia should have some point of view. Since so much of what is said about UFOs depends on the point of view of the writer (not to mention the writer's honesty and competence), the average reader, even one fairly knowledgeable on the subject, is going to have a great deal of trouble evaluating the worth of any particular article. Since each contribution is signed, the reader can check back with the author's position statement, but that doesn't always help very much.

For example, the article on the Travis Walton "abduction" case is written by APRO's Coral Lorenzen. In her own position statement, Ms. Lorenzen makes her strong belief in the extraterrestrial-visitation hypothesis absolutely clear. Fair enough. In her Walton article she states, "Tests indicate that he [Walton] has related his experiences truthfully." Not according to Phil Klass. Klass has charged that APRO, along with the sleazy *National Enquirer*, suppressed the results of a lie-detector test that Walton had flunked. Ms. Lorenzen doesn't have to agree with Klass's charges. She can refute them if she wishes. But not to mention them at all and pretend that Walton's assertions have been unchallenged — that's just not acceptable. The editor should have caught that one. Further, I seriously question assigning an article on such a subject to someone who has been as deeply involved in it as Ms. Lorenzen has.

On the other hand, the article on the 1897 Aurora, Texas, spaceship "crash," which is adapted from the bulletin of Ms. Lorenzen's APRO, clearly (and correctly in my view) labels the whole thing as a hoax. Why the skeptical attitude in the one case but not in the other, which is even more obviously faked? This, I suspect, has more to do with UFOlogical politics than with anything else. Aurora was being pushed by an organization that was a rival to APRO, and APRO did its best to sink its rival's claims. Perhaps in APRO's enthusiasm to get back at a rival they were

guilty of what they have so often accused others of — blind skepticism.

This sort of problem arises constantly throughout the *Encyclopedia*. I found excellent articles alternating with bad ones or those that were just plain silly. I had no way at all to judge many of the articles, because I didn't possess any independent information on the subject and I could not automatically trust what I read. This represents a great drawback in any book that has pretensions to becoming a standard reference.

The editor's preface says that in addition to being a compendium of UFO information the *Encyclopedia* is supposed to be a "fun" book. For the most part it is pretty heavy going, and I doubt if many are going to find it fun. Perhaps the publishers had something like *The People's Almanac* in mind. If so, they didn't get it. That observation leads me to say some nice things about the *Encyclopedia of UFOs*. It is not merely an exploitation of a semi-sensational subject. While it is certainly not "comprehensive and objective," as stated on the cover, it does represent a reasonably serious attempt to present a compendium of UFO information in a single volume. For all its flaws, *The Encyclopedia of UFOs* is the best and most complete work of its type that we have or that we are likely to get for a very long time.

The Encyclopedia of UFOs is overpriced, unattractively produced, intermittently accurate, and generally dull; yet no serious student of UFOs can afford to be without it. A backhanded compliment perhaps, but this is a backhanded business.

The Roswell Incident. By Charles Berlitz and William L. Moore. Grosset & Dunlap, New York, 1980. 168 pp. $10.00

Reviewed by Robert Sheaffer

If *The Roswell Incident* can be taken seriously, it is the story of the century. A flying saucer reportedly crashed in the vicinity of Roswell, New Mexico, during the summer of 1947. The bodies of humanoid aliens were said to have been recovered. There may have been at least one survivor.

Charles Berlitz is already famous for his sensationalist writings about the Bermuda Triangle (see Kusche, *SI*, Fall/Winter 1977; Klass, *SI*, Fall/Winter 1977), and William L. Moore is widely respected in UFOdom. Stanton T. Friedman, who calls himself the "Flying Saucer Physicist," was a research consultant for the book, and his name is scattered throughout. (When I spoke to him at the recent Smithsonian UFO symposium, Friedman said that he was a research consultant to Moore, but not necessarily to Berlitz; he balked at endorsing Berlitz's so-called research but declined to give him an out-and-out repudiation.) Friedman has been trumpeting the book's "evidence" for crashed saucers to the press, saying that, while the other rumors of saucer crashes may be baseless, this one seems to be authentic. Leonard Stringfield, a member of the Board of Directors of MUFON, a

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Robert Sheaffer is a systems analyst, a long-time investigator of UFO claims, and author of the just-published UFO Verdict *(Prometheus).*

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major UFO group, says that the book "cuts through some of the old smoke and reveals some of the fire." If the book has a fault, says he, it is that the authors are excessively skeptical about saucer crashes occurring *after* 1947.

"The majority of space ventures have encountered UFOs," the authors state, and with this remark we can begin our calibration of the accuracy of their claims. (Let us even go so far as to assume that Berlitz has repented of the wild errors of his Bermuda Triangle writings and has decided to "go straight." We will consider only the reliability of the information contained in *this* book.) James Oberg has examined in depth the various claims of astronaut UFOs (see *SI*, Fall 1978) and has found them to be based entirely on exaggerations, inaccurate reporting, and outright hoaxes. "Additional confirmation" of the Apollo 11 astronaut UFO sighting was said to come from an unnamed source "apparently associated with Anglia TV in London." This is an obvious reference to a TV documentary-spoof, "Alternative 3," which claims that a secret space program exists to kidnap innocent persons, lobotomize them, then transport them to the back side of the moon to do manual labor. It was an April Fool's joke; see the *London Times*, June 21, 1977, and *Second Look*, November/December 1979. It is not clear if Berlitz and Moore themselves were taken in by the joke or whether they were attempting to take in their readers.

The newspapers reported in 1947 that a saucer allegedly *had* crashed in Roswell. They then ran follow-up stories saying that the supposed saucer was just the remains of a high-altitude weather balloon. The authors say that this was just a government cover story to hide the truth about a *real* saucer crash. Barney Barnett of Socorro, New Mexico, claimed to have seen a crashed saucer with dead humanoid bodies. Unfortunately, Barnett is dead now, and his wife is, too. But their friends tell a mighty fine second-hand tale. William W. Brazel reportedly discovered the pieces of the crashed saucer on his land. He's dead, too, but his son and daughter-in-law "recall the incident well." It is not clear why the authors of *The Roswell Incident* think that the testimonies of these two departed witnesses corroborate each other, since they place the alleged crash sites more than 100 miles apart. The difficulty is resolved, however, by the speculation that either *two* discs crashed at the same time 100 miles apart or that one disc exploded in midair, scattering debris on the Brazel ranch before it crashed near Socorro, 100 miles to the west. A character witness cited to bolster Barnett's testimony. Holm Bursum, Jr., is a bank executive and former mayor of the town of Socorro. The authors fail to note that, by an astonishing coincidence, the famous Socorro UFO landing of 1964, witnessed by Lonnie Zamora, allegedly occurred on property owned by Bursum. It is remarkable that this man should be involved in both a UFO landing *and* a UFO crash, nearly twenty years apart.

Other sources of information concerning the whereabouts of the saucer after the alleged crash are Desmond Leslie and George Hunt Williamson. Both of these men were close associates of the late George Adamski, who claimed that his friends from the planet Venus took him to the moon in their flying saucer, where he saw forests and cities. The authors note that Leslie had "compromised his reliability" through this association but suggest that his story "may well have been true" nonetheless.

Two other major sources of information on the alleged crash are Riley Crabb and the late Meade Layne, present and former directors of the Borderland Sciences Research Foundation. The authors appear to take the accounts given by these two

men at face value and expect the reader to do likewise. No information whatsoever is provided to enable the reader to judge the reliability of their accounts. Permit me to fill in the gap. The Borderland Sciences Research Foundation has long been associated with the most far out fringe of contactee UFOlogy; their teachings on the ethereal nature of extraterrestrial travel are so bizarre that they make even Adamski appear tame. Meade Layne was the author of works with titles such as *Mystery of the Ethereans* and *The Coming of the Guardians*. J. Gordon Melton, working on a historical study of UFO contactees, says that Riley Crabb "was saying as early as 1957 that all the [UFO] space flights are etheric." Much of the "research" performed at Borderland involves mediums, a fact apparently not noted by the authors or their research consultant, or those UFOlogists who embrace the book's conclusions.

The testimony of Nicholas Von Poppen, a supposed baron, who allegedly photographed the crashed disc at the request of military intelligence, comes to us via Gray Barker, operator of the Saucerian Press. (Von Poppen has also, alas, passed on, and can no longer answer questions.) The reader is not told that Gray Barker had also presented the "Alternative 3" hoax as if it were fact and was the first to write a book on "Mothman," a strange creature akin to Batman of comic book fame. From my own personal acquaintance with Barker, it is obvious that he has one of the most subtle senses of humor in all of UFOlogy. He admits to "spicing up" supposedly factual accounts with pure fiction when the mood strikes him. I am astonished that anyone with any degree of UFOlogical sophistication believes that Barker takes his own writings seriously.

One anonymous but supposedly credible source, referred to as "J.K.," is quoted as saying that "since 1948 secret information concerning UFO activity involving the U.S. military has been contained in a computer center at Wright-Patterson." That statement alone refutes J.K.'s claims about seeing nine dead aliens in the deep-freeze. Given the utterly primitive state of computer technology in 1948, it was almost impossible to store any significant amount of information. The minuscule memory capacities of those ancient vacuum-tube machines made it more likely that data would be carved into stone tablets than kept in the computer. By projecting today's technology back to 1948, J.K. demonstrated his ability to describe things that plainly do not exist.

The authors selectively quote various documents that were obtained from the CIA, the FBI, and other federal agencies via the Freedom of Information Act. Only the items that seem to suggest a UFO cover-up are mentioned, while other documents, suggesting virtually no serious interest in UFOs, are ignored (see Klass, *SI*, Spring 1980). One of the CIA UFO documents not mentioned — the report of the famous Robertson panel — seems to destroy any credibility the Roswell incident may have had. Why would the CIA convene a secret panel in 1953 to discuss what UFOs might be ("no evidence" was the conclusion) if U.S. Intelligence agents had actually confiscated a crashed saucer in 1947?

"On the dark side of the moon [all sides of the moon receive equal amounts of sunlight] the Soviet Luna 9 reported geometric arrangements of huge stones, which, according to Professor Ivanov, a Soviet space scientist, *could* be flight markers for a lunar runway." The final chapter, "The Russian Connection," consists of material like this. It is not at all clear what this unsubstantiated and often inaccurate material has to do with the Roswell "crash." It is likewise unclear what purpose a lunar runway would have on the airless moon.

The widespread acceptance of "crashed saucer" tales such as *The Roswell Incident* provides the best example of what I term the "credulity explosion" now taking place within the UFO movement. Tales that were too wild to be believed when they first surfaced decades ago are now being rehabilitated as credulity stretches far enough to accommodate them. The best summation of *The Roswell Incident* comes from its own pages: "Unfortunately we are forced to rely on rumor again as our best source of evidence." •

Articles of Note

Birx, H. James. "The Creation/Evolution Controversy." *Free Inquiry*, Winter 1980-81 (premiere issue). Excellent review by chairman of Canisius College's Sociology and Anthropology Department.

Brandt, Anthony. "Face Reading: The Persistence of Physiognomy." *Psychology Today*, December 1980. Reports on the return of an old pseudoscience — with some new wrinkles.

Cowen, Robert C. "UFOs: Fact and Frivolity." *Technology Review*, November/December 1980, pp. 6-7. Echoes call of Smithsonian UFO symposium participants for more hard-nosed, no-nonsense investigation and less attention to spurious sightings and sensational talk about "encounters" and "alternative realities."

Emery, C. Eugene, Jr. "The Danville Dowsers Harbor No Doubts." *Providence* (R.I.) *Sunday Journal Magazine*, November 2, 1980. Science writer's examination of the psychology and folklore of dowsing, based on his reporting at the American Society of Dowsers' annual convention in Vermont and queries to dowsing critics. Author was easily able to fool dowsers (e.g., by falsely bending rods down at one point, he induced others to come over and "confirm" his find). Concludes that divining fills a basic human need and will be around for a long time.

Gregory, Anita. "Why Do Scientists Engage in Fraud?" *Parapsychology Review*, November-December 1980. "To experience surprise that scientists should sometimes cheat," cautions this psychologist, "is to forget that scientists are human beings who have aims and purposes and goals other than the dispassionate pursuit of truth." She warns that these other motivations "may become dangerous pitfalls to those working at the edges of science."

Kelly , Ivan. "The Scientific Case Against Astrology." *Mercury* (Astronomical Society of the Pacific), November-December 1980, pp. 135-142. Good review of the scientific evidence accumulated regarding traditional astrology. A subsequent article, in the January-February 1981 *Mercury*, examines the claims of "cosmobiology."

Leo, John. "Memory: The Unreliable Witness." *Time*, January 5, 1981, p. 89. Report on the research of psychologist Elizabeth Loftus showing that remembering is often a creative blend of fiction and fact. See also her article "The Malleability of Human Memory" (*American Scientist* 67: 312-320, May-June

1979) and her new book, *Memory*.

Marshall, Eliot. "Police Science and Psychics." *Science* 219:994-995, November 28, 1980. Inquiry into the way police and press were hoodwinked into giving a reputed psychic free publicity in the Atlanta child-murders case. "City officials are reticent about [Dorothy] Allison's visit, perhaps because they are awakening to a cold feeling that they were gulled."

Schatzman, Morton. "Evocations of Unreality." *New Scientist*, September 25, 1980, pp. 935-937. More insights into the neural physiology of hallucinations. An apparition — a vision of something clearly not there — can nevertheless stimulate electrical activity in the brain the same way real vision does.

Weber, Christopher Gregory. "Common Creationist Attacks on Geology." *Creation/Evolution*, Fall 1980, pp. 10-25. Addresses creationists' distortions of the earth sciences. (This fine new journal — 953 8th Ave., Suite 209, San Diego, CA 92101 — is performing a much-needed public service in straightforwardly answering creationist contentions.)

—*Kendrick Frazier*

Some Recent Books

Fuller, Uriah. *Further Confessions of a Psychic*. Available from Karl Fulves, Box 433, Teaneck, N.J.) 1980, 70 pp., $6.00, paper. More insights into the psychic rackets by a man who knows all the tricks. "Fuller" shows how dozens of them are done.

Neher, Andrew. *The Psychology of Transcendence*. Englewood Cliffs, N.J.: Prentice-Hall/Spectrum, 1980, 361 pp., $7.95, paper. An excellent work demystifying mystical and transcendental experiences. Psychologist Neher shows that "mystical" and "psychic" experiences often have normal physiological explanations that avert the need to resort to "paranormal" hypotheses. Forewords by Robert Morris and Ray Hyman.

Sprague, Roderick, and Krantz, Grover S., eds. *The Scientists Looks at Sasquatch (II)*. Moscow, Idaho: University Press of Idaho, 1979, $6.95, 195 pp., paper. Essays and articles of variable approach and quality.

—*K.F.*

Follow-up

Beckjord on Bigfoot

I don't know if there is enough space to respond to all the errors and misrepresentations in Paul Kurtz's article "Bigfoot on the Loose" in the Fall 1980 SKEPTICAL INQUIRER (I found more than 25), but I will try.

Leading the way, and indicative of the poor memory of Paul Kurtz as an investigator, is the interesting claim that I am a bearded man. Even though Dr. Kurtz sat and debated with me for over three hours, he fails to remember that I was clean-shaven, as I have been for several years and still am. Little wonder, then, that the following goofs appear.

The "media" usually followed my press releases, and the majority stated that I was in Lewiston to search for physical remains of a strange animal that had appeared in some photos that had been sent to me. None to my knowledge stated that Bigfoot had been found or proved.

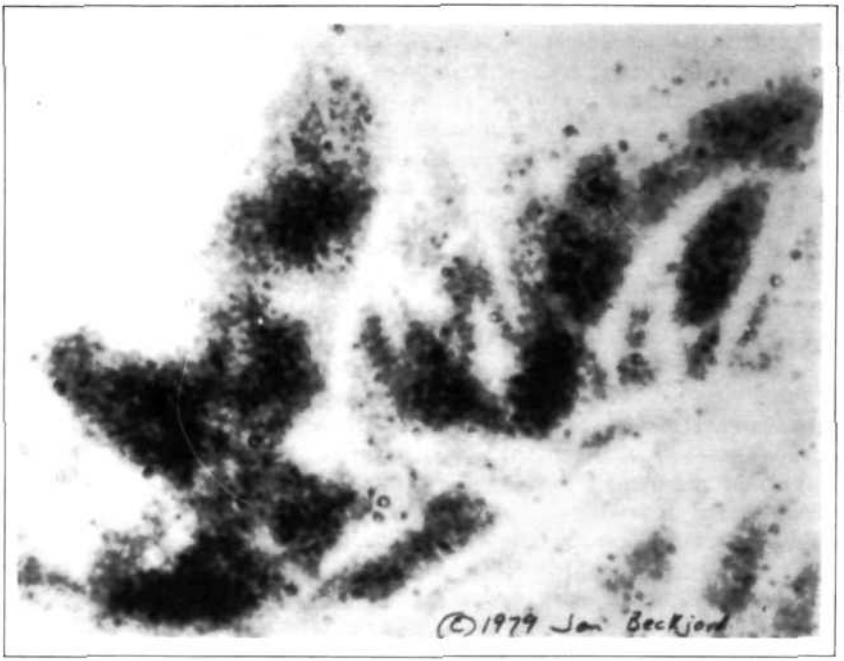
Next, I invited Kurtz et al. to come to Lewiston to view the photos we had of this strange beast and to meet persons who had seen it, as well as others who had seen other Bigfoot-like animals of varying sizes in the area over the years. I never claimed we had satisfying evidence, which, as any student knows, consists of bones or a body, and nothing less. I was in Lewiston to see if such could be found, but we hadn't found any yet.

Next, Project Bigfoot is out to seek such physical proof; we never told Kurtz or CSICOP, or anyone else, that Bigfoot or Sasquatches solidly exist on a scientific basis. Next, I've never claimed that definitive evidence was on hand from us. I *have* called for the formation of a subcommittee on Bigfoot to examine claims and to investigate on its own. Kurtz, Cazeau, and Scott unknowingly became such a subcommittee for one day.

The much enlarged photos I sent to Kurtz have been analyzed by a team of four off-duty U.S. Army photo-interpreters, skilled in spotting disguised troops in photos, and their conclusion was that they saw "monkeys," "apes," "gorillas," "dogs," "a catlike animal" and "objects we can't explain away."

Three hunters, not *two,* all introduced to Kurtz et al., had encountered a body that was missing only its intestines and that was approximately five feet long, a small carcass for a bear, if it was one. The head and rear "feet" were cut off and taken to a parking lot next to a firehall. A policeman failed to act on his inner impulse to impound the remains, and the parts were lost later through carelessness.

Next, *the carcass was never picked up* and *never brought to the Buffalo Museum of Science.* Only a bag of old animal skin and hair, found at the same site



Jon Beckjord

Editor's note. The following description of the photo above was supplied by Jon Beckjord: "'Big Mama' Sasquatch female, shown in squatting position with knee and one leg visible, torso with breasts and grinning head. Taken from a color slide by Jon Beckjord and enlarged 90 times, then converted to black and white. This is one of the most easily seen of a group of what may be paranormal animals that inhabit a certain area in the Sierras near Lake Tahoe at 7,000 feet."

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many months later, was brought to the museum, along with photos of the head and feet. Clark and Dorr, whom I interviewed in Buffalo, did identify the skin and hair as that of a bear, and they also speculated that the photos showed a dead bear. No one can be sure there is a connection between the photos and the old skin and hair in the bag.

Policeman Filicetti, not I, contacted the media and started a media event prior to my arrival. I had not wanted this; but, once started, I gave interviews on the progress of our investigation in order to stimulate someone, anyone, who might have found the remains of the missing creature to come forward with them so that competent scientists could have a good look. Kurtz has a number of our press releases. He of all people should know that I never claimed we had solid physical proof of Bigfoot, i.e., bones or a skull.

Next, I've never been to Florida to investigate a sighting. However, calls have come in from Florida regarding sightings and tracks. Actually there are as many sightings there every year as there are in the Northwest. I told the media that the Lewiston event, if we could find the skull or feet, would possibly prove Bigfoot, but that in the meantime the *photos* were the best of Bigfoot—if in time we were able to prove *what* they were of. Again, Kurtz, from his experience with the press, should know better than to expect accurate quoting when paranormal events are involved. I don't believe everything I read in the papers, and I hope he doesn't either.

In regard to our hearing, which, Kurtz termed a "commission of inquiry," he

ignored the statements of a local taxidermist, who had stuffed many black bears, to the effect that in his opinion the photos did not show a bear. He even brought a frozen black bear's head and skin to the table. The differences were considerable. The possibly collapsed nose of the creature in the photo was flesh-colored, yet the black bear's was solid black. The photo showed no visible ears, and the hunters had all testified that they had found no trace of any decayed stumps of ears or any chewed-off stumps. The real bear's ears stood up high for all to see. Kurtz also ignored my testimony that the tooth sizes for the canines were of the Kodiak bear size, or the Alaska brown bear size, which mammologist Dr. Sidney Anderson of the American Museum of Natural History later confirmed, and far exceeded typical measurements of those of black bears. What was an errant Kodiak bear with a five-foot body doing in New York State, if it was a bear at all?

Next, the daughter of the town clerk, a young married woman with a reputation for level-headedness, had given us a copy of a drawing she had made of an animal that she had seen *twenty feet* away (not "crouching in the distance"), which she said was similar to the photos of the dead animal. We brought forward three other persons who had seen Sasquatch-like creatures in the area. These creatures were always bipedal and large, and—in response to Scott's objection about a lack of multiple sightings (several Sasquatches together)—of varying sizes. Different sizes of animals, as well as colors, indicate a breeding population, even for paranormal beasts. Since I had no solid physical remains, I always was careful to say I felt that the photos did not show a bear; but I continually qualified my statements by saying that we were seeking physical proof and that we intended to submit the photos to experts at the AMNH and the Smithsonian (we did) for their opinions. (And they felt, Anderson and Thorington, that the animal was a bear, albeit a strange one because of the excessively large canines for an animal found in black bear range.) The snout of the animal was stubby and short in comparison with a black bear's snout. The hunters, the firemen, and the taxidermist had all remarked on that. Scott, Cazeau, and Kurtz were not woodsmen or zoologists, and their disagreement on this point was taken poorly, to my amusement, by the locals, who tended to back the taxidermist.

After Kurtz, Cazeau, and Scott had finished, pronounced the photos to be of a black bear, and departed, I did indeed learn that the hairs that I had located at the rural dump site might have been of some other animal than the one in question. To avoid any mistakes, I called Cazeau at the Geology Department at SUNY-Buffalo to tell him that the hairs I had given them might, or might not, have come from the strange beast. My diligence was rewarded by having Cazeau hang up on me!

We never did find the remains that we sought. Thus the event stays a mystery. The hunters found a strange creaure that looks in some ways like a bear and in other ways not. Under the rules of science, since bears are classified and Sasquatches are not, a scientist is safe in calling it a bear. However, in checking out this tantalizing story, we managed to interview five local citizens who had seen animals that looked like what Sasquatches are alleged to look like. None wanted fame or money; most had to be coaxed to step forward.

What does this attempt of Kurtz to discredit my efforts to bring out the public in the Lewiston affair show? What wrongs are we doing in our campaign to encourage persons with knowledge of possible "Bigfoot" activities to come forward and tell their stories of encounters? Why does Kurtz object? His strongest language in the article comes when he refers to my appearance on national television ("Good

Morning America"). I have the video cassette of that appearance, and I always stressed the "maybe," "possibly," "we are checking," "we are seeking proof" aspects. I never said that Bigfoot existed in fact, only that we are trying to find out and that we hoped watchers would help us. *Here is the sore point with Kurtz and the CSICOP.* Kurtz's article stresses that the growing media attention to the claims coming from Lewiston was his reason for coming out, vigilante style, and scotching the heresy before it got too big. A professor of philosophy, a professor of geology, and a professor of anthropology (not a zoologist) came down to Lewiston to pit their wits against a nonscientist investigator, a fireman, a teenager, and a taxidermist, hoping to quench the flames of the growth in paranormal belief.

For myself, I found that the spirit of inquiry was stronger in the young and in the nonscientists than in the actual scientists in this case. To these amateurs, the case was not closed before they even got out of their cars—they did not hold the attitude that such an animal or animals *could not* exist and that it was simply a matter of finding the right normal explanation that fit. Instead, they have pressed on and have collected a number of other sighting reports from other citizens of the area that seem to indicate that the alleged Sasquatches are international border-crossers that have no fear of the Love Canal. It will very possibly be through the efforts of such untrained young people, working with the media, that science is ultimately presented with a set of bones that will prove the existence of Bigfoot and his kin. And the Lewiston events will, at the very least, have alerted the country *to not throw out the remains of strange-looking animals.* I say this is a positive approach to science, and a far more positive effort than biased debunking.

—Jon Beckjord

Responses to Beckjord

It is worthwhile to have Beckjord's admission in print—contrary to what he claimed to the media and insisted to us while he was in Lewiston—that no "satisfying evidence" has been found that would point to a Bigfoot in the Lewiston area. It is also important to have Beckjord now state, "We never told Kurtz or CSICOP or anyone else that Bigfoot or Sasquatch solidly exist on a scientific basis . . . I never claimed *definitive evidence* was on hand from us" (emphasis added). If that is the case, then he should cease and desist from claiming all over the land that "evidence" for Bigfoot has been uncovered.

Beckjord maintains that the media misquoted him. But we have seen dozens of news clippings in which he states that he does have evidence for Bigfoot. For example, a clipping just received from *Sacramento* magazine (April 1980) says, "If Jon Beckjord of 'Project Bigfoot' in Seattle is correct, the more remote areas of Lake Tahoe may be home for literally thousands of the legendary creatures . . . This 'evidence' appears in photographs taken by a bay area woman . . . Pictures revealed up to 50 apelike creatures . . . According to Beckjord, 'One picture shows two blond-headed anthropoids holding two babies.' Beckjord's explanations are . . . that the animals are interdimensional and move from one space-time continuum to another . . . and that they can exist in an electromagnetic area outside of the visible spectrum of humans, but not cameras." This only reinforces our view that Beckjord is as loose in his interpretations of "evidence" as the creature he is relentlessly pursuing is in roaming the country. Beckjord is so eager to make his case that he

tends to stretch the facts to suit himself.

Beckjord is correct on one point: he did not have a beard. Perhaps I saw only a five o'clock shadow. However, there were two hunters at the hearing (not three) and they testified that the alleged carcass was brought to the Buffalo Museum of Science, where it was identified as a bear by the chief taxidermist and assistant curator. Incidentally, we have never denied the possibility of Bigfoot, but one should not confuse possibility with proven fact.

—Paul Kurtz

Mr. Beckjord, like any other member of society, should be free to indulge his particular intellectual passion, in this case Bigfoot monsters in Niagara County, New York. We have a right to object though, when Beckjord's extravagance, in the name of scientific inquiry, misstates both particulars and generalities.

The Niagara County "evidence" was a buffet table of dubious photographs from western North America, a bear skin, and other irrelevancies. Like other paranormalists, Beckjord makes a number of cautious disclaimers to certainty at appropriate times, but otherwise obliges his nonprofessional audiences with a take-it-for-granted, absolutist belief in Bigfoot. He does little to encourage rational social discussion of the subject. In place of the logic of explanation, he fends off critics with emotional accusations of a lack of scholarly integrity. In the Niagara case, he casts aside the CSICOP "subcommittee" as comprising nonzoologists and nonwoodsmen, placing a higher premium on the "level-headedness" of a young married woman, a fireman, and a taxidermist. Could it be that Beckjord will accept only the testimony of those who agree with him, regardless of their background?

Let's hope that public awareness in Niagara County includes some weight of scientific tradition instead of the simplistic view of a Bigfoot lurking in Niagara's vineyards.

—Stuart D. Scott

The photographs I saw were those of a bear. Contrary to Beckjord's assumption, I am a woodsman (having been a geologist for 30 years) and have seen many bears; I was even chased by one in the Adirondacks. The "eyewitness" testimony was weak indeed. The idea that a family of strange creatures of large size could live undetected and uncaptured is foolish. A bunch of elephants in the Denver suburbs would be more elusive. Now Beckjord says Bigfoot is invisible. Come on. Maybe there is a Bigfoot, but he doesn't reside in Lewiston or the Internal Revenue Service would have found him long ago.

—Charles J. Cazeau

Cutting the Deck with Susie Cottrell

My purpose is not needlessly to pursue the recent interchange with James Randi (*SI*, Spring 1980, Summer 1980) but chiefly to present further data about the claimed abilities of Susie Cottrell. I shall nevertheless take as my point of departure Randi's statement (*SI*, Summer 1980) that "when we merely cut the deck — a

procedure established as a wise and prudent move in card games — she [Cottrell] was unable to perform the trick." This statement, as it stands, is obviously not one from which one can infer cause and effect in regard to the variable in question (cutting the deck). To throw some statistically meaningful data about this into the situation, therefore, I would like to present briefly two striking instances of a procedure used during the 1979 testing of Cottrell in Denver where cutting the deck, which was accorded a special significance in the evaluation of the results, *did not* result in failure.

Both instances took place during a preliminary warm-up session on January 21, 1979, when a group of investigators were trying to work out the procedural details of the tests described in the Spring 1980 issue of the SKEPTICAL INQUIRER. On this occasion, a number of trials of the two procedures we had elected to test —the selection of a designated card from a deck dispersed on a revolving turntable and Susie's electrodermal response to someone else's selection of a designated card — had been carried out. Present, besides two cameramen and myself, were Professor Peter Phillips, of the Department of Physics, Washington University, St. Louis; Raymond M. Wainwright, professor emeritus of electrical engineering, University of Denver; and Merle Worth, a New York documentary-film producer. A turntable trial had just been completed, with me as selector. Susie was seated at the bridge-table upon which the turntable rested; Phillips was on her right, and I was on her left. Wainwright and Worth were seated a couple of feet away from the table to my left. We were about to break for lunch when Susie suggested that we try a procedure with which she had had (she claimed) a high rate of success. After designating the nine of clubs as the target card, she requested me to shuffle the deck we had been using. She then instructed me to hold the deck face down under the table, out of her line of sight, and to peel off cards slowly, one by one, from this position, placing them face up on top of the 21-inch revolving turntable platter. I was told that I could cut the deck at any time I pleased during this procedure. After the twentieth card, I did cut, and at the same time announced that I had. After the thirty-fifth card, Susie called out "Stop." I asked why. She stated that the next card would be the nine of clubs. I duly peeled it off and threw it onto the turntable. Nine of clubs it was. In continuance of what had been done up to this point, the entire procedure was videotaped by two cameras.

An attempted repetition immediately afterward was a failure, although just barely so, as was another one about ten minutes later, with Worth at her own request replacing me at the table as shuffler and dealer. (In between, 5 trials of a hitherto untried turntable variant suggested by Susie were carried out.) Immediately following this, however, there was a second successful trial, with Worth again as shuffler and dealer. (Phillips and Wainwright remained where they had been, while I had seated myself away from the table to the left of Worth and the right of Wainwright.) In this instance Susie designated the six of diamonds *after* Worth had completed her shuffling under the table but before she began to peel the cards off. This was then done and the cards placed face up on a 16-inch-high pedestal table that had been placed under the bridge-table. (One of the cameras focused on the cards being placed on this low table, and the other focused on Susie and Worth above the higher table.) A cut was made and announced by Worth after the eighth card. After the fifteenth card Susie stated that the next card would be the one. The camera records this card being peeled off and placed on the pedestal table. It was the six of diamonds. At this point we broke for lunch.

Because of our decision to concentrate on the two procedures mentioned earlier (and described in the Spring 1980 SKEPTICAL INQUIRER), the under-the-table cutting procedure was not repeated during the remainder of the January 21 session, nor during other sessions with Susie through March 18, after which no further sessions were held. There would nevertheless have to be a lot of misses to outweigh the 2 hits in the 4 trials that were carried out on this one occasion, since the probability of these hits having occurred by chance (any question of optional stopping aside) is about .002. For it to be justifiably claimed, moreover, that cutting the deck might be responsible for Susie's failure in any situation where she claimed or demonstrated a comparable success rate without cutting, a significant difference would have to be established between cutting and no cutting in a suitable number of randomized trials of each condition. There has been no indication that anything like this was ever done.

—Jule Eisenbud
Denver, Colorado

James Randi responds in the following communication.

More Card Tricks from Susie Cottrell

Jule Eisenbud, the discoverer of former psychic star Ted Serios, the "thoughtographer," has bounced back into the fray with a most revealing account of a performance by Susie Cottrell, another conjuror who has fooled the good doctor. His latest submission should serve, once analyzed, to settle the discussion for good. His article above is, I think you will agree, excellent proof of Dr. Eisenbud's inability to see through the simplest of ruses. Read it carefully, then consider my comments.

His article is important because it illustrates that:

1. Eisenbud ignores the facts of the CSICOP tests of Cottrell, in which (*a*) she was caught blatantly cheating and was recorded on videotape doing so, to the entire satisfaction of experts who viewed the evidence, and (*b*) she failed to do the trick when the deck was cut — the *only* procedure needed to circumvent her method of performing the trick. Otherwise, the protocol was exactly the same.

2. Eisenbud *allowed Cottrell to design the experiment*, doing it *her* way. *She* chose the card that would be discovered and *she* gave the explicit instructions to the experimenter.

3. Eisenbud supplied the rationale for her failure ("just barely so") without realizing the import of that near miss! As we shall see, a near miss is entirely congruent with the method used in this trick.

4. He applied statistics to a faulty experiment and implied that this adds validity to the procedure. It is much like buying a very expensive, silver-plated bucket for carrying water — and discovering there is no bottom in it.

Where, then, does the bottom fall out? Right at the start. Recall that Susie had been handling the deck before announcing her experiment. All she needed to do was peek into the deck and remember two consecutive cards in order, from the top down. Suppose she spotted a two of spades followed by the nine of clubs. That's all the information she would need. But — most important — she would have to stick

with that second card as her target and insist that the cards be taken from the deck in order, one at a time, from the top. Cutting would in no way interfere with the sequence, except in the unlikely event that it exactly split the two memorized cards, in which case the very next card dealt after the cut would be the chosen one, and a further miracle would be claimed! Note: The cards *were* taken from the top, and cutting *was* allowed...

But what about the shuffle? Surely that will disturb the cards? Not necessarily. Try it yourself. A simple riffle shuffle need not separate any given pair of cards, and if it does, it usually inserts *one* card between them. Overhand shuffling can easily fail to separate the two, or disturb the one-in-between situation.

Note that last situation. Susie's method is obviously to watch for the "key card" — the two of spades in this case. If she announces that the nine of clubs will follow it, and it is *two* cards up ahead rather than only one, that can be designated a "just barely" missed guess! Well, how thorough was the shuffling? To find out, I called Merle Worth, the film producer who participated.

Understandably, Ms. Worth had forgotten much of what was less than a memorable experience. She did recall that, during the rest of the "interminable" tests that Eisenbud conducted with Cottrell, he kept changing the rules and conditions. As for the shuffling used in the test described here, she recalled that it was perfunctory and that her own shuffling is very amateurish and consists of simple overhand mixing. Such a process quite naturally leaves a lot of pairs together. She does not ever use a "riffle" shuffle, which might insert a card between the pair.

Remember, all that is necessary for a perfect success is that *one* pair of cards in that deck — previously noted — be left together. And a "bare" miss — half a success — results from a card finding its way between that pair.

Sure enough, further information supplied by Eisenbud verifies for us that in the two "failures" the chosen card was exactly *one removed* from the point at which Susie called, "Stop!" *This is a result predictable from an assumption of the modus operandi described here!*

It is well to note that when we tested Cottrell in Buffalo, the cutting of the deck was sufficient, since we were aware of, and had guarded against, several other methods that might have been used to cheat. That was a specific test, the nature of which was known in advance. In Eisenbud's test, as described by him, cutting the deck had nothing to do with it! How he can expect to apply such a precaution to an entirely *different* card trick is beyond me.

Finally, a simple question: Eisenbud used a physicist, an electrical engineer, and a film-maker as witnesses to these latest conjuring demonstrations. Why, then, did he not use a conjuror, who would have guarded against these flummeries? The answer, I think, is that Eisenbud simply does not *want* to know the truth about Susie Cottrell — or about his own fuddled thinking processes. I leave him to his delusions.

What he has done, in attempting to fault the procedures and conclusions of the CSICOP investigation of Susie Cottrell, is to supply another example of the kind of simple grade-school card tricks that Cottrell uses to fool people.

—James Randi
Rumson, N.J.

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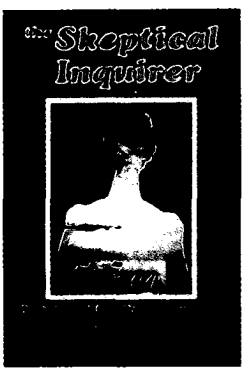

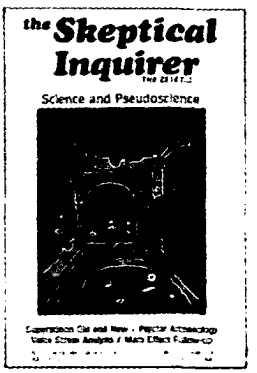

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From Our Readers

The letters column is a forum for views on matters raised in previous issues. Letters are welcome and are more likely to be published if they are typed and double-spaced. Some may have to be edited.

The Velikovsky affair

I found it somewhat ironic that my piece on the unproductive nature of the Velikovsky controversy appeared in conjunction with an article (by James Oberg) that illustrates only too well some of the deficiencies of the arguments (on both sides). The critics of Velikovsky continue to damage their case with bemusing errors of fact. Oberg, for example, is in error on the following points.

Macmillan did not publish *Worlds in Collision* through its textbook division.[1] Velikovsky did refer to the works of Whiston, Donnelly, Bellamy, and Hoerbiger: the index to *Worlds in Collision* shows references to the first two;[2] and there are further footnotes in the text, in places not mentioned in the index, citing Whiston, Donnelly, and Bellamy.[3] Hoerbiger and Bellamy are referred to in *Earth in Upheaval*.[4] Martin Gardner commented that Velikovsky did not make clear the extent to which his work draws on that of Donnelly and Whiston, in general conception and in detail;[5] that seems to me a fair comment. Oberg's statement, however, is simply wrong.

It is also incorrect to say that, according to Velikovsky, the encounters of Earth with Mars were "equally" as devastating as those between Earth and Venus; the former are described as less devastating.[6]

Both sides in the controversy continue to be guilty of such debating tricks as ascribing motives to their opponents and making interpretations which, though perhaps correct, are not based on tangible evidence: for example, Oberg's guess that Velikovsky "turned to ancient myths for intellectual refuge."

Another point on which the opposing sides argue from ideology rather than fact is the matter of the reception by scientists of startling new discoveries. Oberg points out that Velikovsky and his supporters have made far too much of this phenomenon of resistance; on the other hand, the critics of Velikovsky have erroneously attempted to argue the case that such resistance is merely an occasional aberration; for example, Oberg's quote of Michael Jones that "the appearance of new evidence constantly causes scholars to modify their views." The question of how science adapts to revolutionary changes is far too interesting and complex to be accurately described in the terms used by either side in the Velikovsky controversy; a splendid summary, supported by numerous references to actual events, was

given nearly twenty years ago by Bernard Barber.[7]

If we wish to combat pseudoscience, and to demonstrate those virtues of accuracy and objectivity of which scientific work and writing are capable, then errors and oversimplifications of the sort I have mentioned above must be eschewed. Technical experts and writers of popular science need to emerge from their glass houses before they throw stones and to be careful that they are not being potty when they call a kettle black.

Henry H. Bauer, Dean
College of Arts and Science
Virginia Polytechnic Inst.
and State University
Blacksburg, Va.

Notes

1. See, for example, *My Life in Publishing*, by Harold S. Latham (New York: E. P. Dutton, 1965), pp. 71-77.

2. Index to Doubleday hard-cover edition of *Worlds in Collision* cites references to Whiston on pp. 42 and 330 and to Donnelly on p. 42.

3. Not referred to in the index are mentions of Donnelly in footnotes 8 and 10 on p. 118, footnote 8 on p. 122, and footnote 25 on p. 131; and citations of Bellamy in footnotes on pp. 71, 90, 112, and 237.

4. Index to *Earth in Upheaval*, Laurel edition (Dell, 1968), shows references to Bellamy on pp. 86 and 87 and to Hoerbiger on p. 86.

5. Martin Gardner, *Antioch Review* 10 (1950): 447.

6. For example, "Although Mars encountered the Earth more times than Venus ... Mars was much less destructive," in *The Age of Velikovsky*, by C. J. Ransom (Kronos Press, 1976), p. 38.

7. Bernard Barber, *Science* 134 (1961): 596.

I congratulate you on one of the cleverest hatchet-jobs I've seen in a long time. I refer to your treatment of Dr. Immanuel Velikovsky in your Fall 1980 issue. It is clever because its tone is so conciliatory; yet its statements, by and large, are both insinuating and insulting.

Velikovsky *never* "returned the feelings" of denunciation and vilification by his critics, as James Oberg claims. In fact, he repeatedly bent over backward to avoid unjust rebuttals against his critics.

Henry Bauer's essay is so full of distortions and innuendo that it scarcely deserves comment. Yet I must protest strongly his statement that Velikovsky's supporters "want scientists to behave impeccably, in accord with the highest ideas regarding scientific activity, and to reform their ways then they do not." Why shouldn't we? Scientists, in general, have not done so in the past thirty years regarding Velikovsky.

As for Kendrick Frazier, he lost any respect I might have had for him when he referred, in his very first sentence, to Velikovsky's work as "an eccentric brand of planetary-origin theory." It is no wonder, then, that he mentions only the views of those scientists who do not agree with Velikovsky's.

Dominick A. Carlucci, Jr.
Elmsford, N.Y.

In his "Ideas in Collision" (*SI*, Fall, 1980) James Oberg states "By the fall of 1940, as Nazi warplanes decimated the British Royal Air Force over London and nothing seemed to stand between Hitler and world domination ..."

For some of us, it is still a little early to start changing that part of history. It was the outnumbered Royal Air Force (RAF) fighter pilots who successfully repulsed the German Luftwaffe in 1940, and, in effect, stood between Hitler and world domination. It could even be that, without them, crackpot Nazi science would not be universally practiced, and the SKEPTICAL INQUIRER, if it existed at all, would be an illegal and bleak underground newsletter.

J. Richard Greenwell
Tucson, Ariz.

The Frazier and Oberg articles in the Fall 1980 *SI* are among the most lucid, even-handed, and informative pieces that have appeared on the subject of Velikovsky. Bravo!

As a long-time follower of, and participant in, the Velikovsky controversy, I look forward to seeing what other light your journal can bring to the subject.

David Morrison
Institute for Astronomy
University of Hawaii
Honolulu, Hawaii

Recognizing the outstanding contribution of Immanuel Velikovsky to the literature of modern pseudoscience, I propose that the psychological profession formally classify pseudoscientific activity as the "Velikovsky Syndrome."

Gerald McHugh
Baton Rouge, La.

Physicists and ESP

You may be interested in the origins of our piece on ESP, which you criticized in your well-written column in the Winter 1980-81 issue ("Articles on the Paranormal: Where Are the Editors?"). As a lifelong skeptic in these matters, I became curious upon hearing that scientists outside the realm of paranormal research were even acknowledging the possibility of ESP. I had read innumerable anecdotes about ESP, but had never found anything more than the vaguest notions on *how* such phenomena could exist. I wanted to know just what kind of intellectual framework, however tentative, these scientists were postulating. After all, here was I, a nonscientist, assuming that all this parapsychological business was patently *not* possible. The question of *whether* such phenomena do in fact exist was not addressed in this story, and for good reasons, in my opinion. First, I assume *Science Digest* readers are fully aware that there are legions of skeptics who do not believe in ESP. Second, I have seen few intelligent arguments made on behalf of ESP. Most favorable articles about the subject consist of anecdotes and opinions. Here was an opportunity — and it was solidly within an admirable journalistic tradition — to present a nonestablishment point of view. Third, I assume that our readers would have the same curiosity that I did about how scientists would rationalize the existence of ESP.

In assigning the article I chose a writer who was skeptical about ESP to begin with. I had it edited by skeptical editors and checked by skeptical physicists, who made suggestions that I included in the text.

To sum up, the important point to be made is this: *Science Digest* is not the kind of publication that attempts to mislead its readers. Our purpose is to excite our readers' imaginations and intellects, to help them see the world in new and different ways, and to have fun in the process. Our readers are able to make the fine distinctions that enable them to distinguish between an article that attempts to prove that ESP exists (which ours did not) and one that presents stimulating arguments.

Scott DeGarmo, Editor
Science Digest
New York, N.Y.

Academia and the occult

The article by J. Richard Greenwell on "Academia and the Occult" (*SI*, Fall 1980) paints a rather gloomy picture of at least one group of American college students. Greenwell administered a questionnaire on paranormal phenomena to his fellow students in a class on "Witchcraft and the Occult" at the University of Arizona and discovered that these students had very strong beliefs in these phenomena as compared to the respondents in a 1978 Gallup poll.

	Rutgers University (N=21)	University of Arizona (N=28)	Gallup
Ghosts	19%	64%	11%
Clairvoyance	57	71	24
Precognition	62	79	37
Telepathy	48	86	51
Bigfoot	0	32	13
Loch Ness Monster	5	32	13
Astrology	19	46	29
UFOs	33	68	57
Demons/Devils	14	21	39
Angels	29	32	54

I teach a history course at Rutgers University on "Witchcraft and Magic." Out of curiosity, I gave my students a questionnaire along the same lines as the one used by Greenwell. The results were encouraging: my students were consistently more skeptical than Greenwell's and, in seven out of ten cases, more skeptical than the Gallup poll respondents. (See table above.)

Of course the results should themselves be viewed with a bit of skepticism. Among other things, they may have been distorted by the attitude of the teacher (I have taken a debunking position) and by the small number of students polled. Nevertheless, it does suggest that not all college students are persuaded by the claims of the paranormal.

Marc Mappen
Assistant Dean
University College
Rutgers University
Newark, N.J.

Psychologists and ESP

Padgett, Benassi, and Singer (*SI*, Fall 1980) speculated upon possible reasons that psychologists have been found to disbelieve ESP more than respondents in other academic fields. While it might be true that psychologists' skepticism is attributable to greater familiarity with ESP literature, the authors failed to entertain an equally plausible hypothesis: Psychology is chock full of theories as vague and empirically barren as those of ESP. The big difference, of course, is that traditional psychology has received a stamp of scientific acceptance that ESP and other implausible areas have not.

I suggest that psychologists tend to doubt the veracity of ESP so as to protect the legitimacy of their own (questionable) beliefs about human behavior. Like the panhandler who warns an interloper, "*I'm* working this side of the street," so, too, may some psychologists feel that their descriptions of an amorphous, disembodied mind populated by high-sounding but vacuous jargon needs no competition from the ESP crowd.

Mark B. Fineman
Assoc. Prof. of Psychology
Southern Connecticut State College
New Haven, Conn.

Professors and parapsychology

Regarding "Parapsychology Research: Interview with Ray Hyman" (*SI*, Fall 1980), here we go again. Ray Hyman is not a "fair-minded critic of parapsychology," as is stated. If he were, he would

flatly say that in their whole history the parapsychologists have not come up once — not one, single, solitary time — with a legitimately constructed, effected, and analyzed experiment. Instead, because some of the parapsychologists are professors, and because they cloak their bull in scientific terminology, Professor Hyman hasn't got the guts to hit them hard, hit them everywhere — and keep on hitting. All the parapsychologists have ever done is to segregate runs.

Once again, I accuse *SI* of ducking the real issue — that when professors talk nonsense, the other professors will not define it as such. It's hopeless. It's like getting doctors to testify against doctors, or police against police.

Mortimer T. Cohen
New York, N.Y.

Necessary distinctions

I want to register my wholehearted support for the views expressed by Professor Hyman in the interview with Dr. Mishlove (*SI*, Fall 1980) and in his postscript to the interview.

Hyman is right in insisting that the skeptics (of whom I happen to be one) should deal with the evidence at its best when attempting to pass judgment on parapsychology as a whole.

Of course the critics (and the critical parapsychologists) must deal with the nonsense too. After all, much garbage is sold to the public as "scientific parapsychology." But the responsible skeptic should take great pains to avoid giving the impression that he is holding the Helmut Schmidts and the Martin Johnsons responsible for, say, the contents of Puharich's biography of Geller.

It has always been my view that the CSICOP should not only debunk the preposterous claims made in the name of "future science" but also promote understanding of the sophisticated efforts by some parapsychologists to overcome the problems inherent in their field.

Some supporters of the skeptical cause, alas, fail to make the necessary distinctions. For example, I was appalled at Professor Hammerton's letter (*SI*, Summer 1980) in which he implied that the very fact that Dr. John Beloff was invited to contribute an article to *Encounter* constituted a victory for the forces of unreason.

I am happy that Professor Hyman has once again emphasized the difference between critical investigation and ritual denunciation.

Piet Hein Hoebens
Amsterdam, The Netherlands

Innocent "psychics"

I was busy elsewhere when the Steve Shaw fiasco took place in Pennsylvania ("Downfall of a Would-Be Psychic," *SI*, Fall 1980), but we can be grateful that Professor McBurney and Jack Greenberg were available to get the goods on him. Kids like Shaw have been flummoxing investigators in laboratories by appealing to their gullible acceptance of the youngsters' presumed innocence and inability to deceive older, experienced observers. The exact opposite is the truth.

It would appear that Shaw's career as a psychic is effectively ended with this exposure, but these characters have a "rubber-duck" unsinkability that is incredible. Such exposures have a way of being lost in the shuffle. Let us hope that investigators are warned to beware of youngsters who exude innocence and purity while using clever and effective methods to achieve what the inexperienced may consider to be genuine psychic powers.

James Randi
Rumson, N.J.

NBC's Bermuda Triangle

I just started reading my first issue of

the SKEPTICAL INQUIRER, and it's great. It's something the public has needed for a long time.

It was therefore with interest that I watched "The Bermuda Triangle," which aired here on NBC on November 22, 1980. If you haven't seen this one, you must do so. NBC is deserving of another "Uri award."

The documentary was presented in a *real* sense. Among the proposed explanations for the disappearance of ships and airplanes were: a sunken Atlantean magnetic device that draws power from the stars; flying saucers complete with power beams; a mysterious cloud that surrounds a ship, draining energy, including the heat of the earth, leaving things frozen on a hot summer day; some so-called physicist's proposal that a plane passed through a time warp, causing it to miss its landing site, which had not yet been constructed.

I commend your debunking effort. These things are humorous, but the damage such programs can bring, via gullible people, to present and future generations, is obvious, however difficult to assess.

Robert Kabat
Saginaw, Mich.

Mutilations and UFO witnesses

Two additional observations can be made regarding Kenneth Rommel's report on alleged "animal mutilations" (*SI*, News and Comment, Fall 1980).

(1) In some cases Rommel found substantial "errors in official reports" on mutilations. Experienced UFO investigators have encountered the same problem with "official" reports in many UFO cases. In one such instance the UFOs reported by witnesses turned out to be aircraft engaged in aerial refueling operations, but Air Force representatives had mistakenly advised the witnesses that aircraft could not account for their observations. (Details of this case are discussed by Roy Craig in a section of the Condon Report delightfully entitled "Misinterpretation Supported by Official Misinformation." See E. U. Condon, *Scientific Study of Unidentified Flying Objects*, Bantam, 1969, pp. 65-70 and pp. 334-339.)

One seemingly baffling UFO encounter, involving both radar and visual observations, was recently found to have been based in part on erroneous information in the official Project Blue Book case-file. (Details in Ian Ridpath's article "New Light on Lakenheath" in Hynek's *International UFO Reporter*, August 1978, pp. 6-7.)

Credulous as they are, many UFO proponents appear to believe that, if a report is stamped "Official," its contents *must* be true and accurate. For example, in his most recent book Jacques Vallee informs his readers that the "mutilation" reports he accepts as legitimate and mystifying are those that were investigated by law officers, a veterinarian, and a coroner (Vallee, *Messengers of Deception*, And/Or Press, 1979, p. 162). To be sure, this establishes that those reports are indeed "official." Unfortunately it does not establish that they are accurate or complete, but Vallee does not bother to make such distinctions.

Finally, what UFO enthusiast could ask for a more official UFO report than that filed by President Carter himself? And yet as Robert Sheaffer has shown (*Humanist*, July/Aug. 1977, pp. 46-47), details of Carter's experience were misreported and the "UFO" proved once again to be an object that J. Allen Hynek says thousands of people can't tell from a hole in the ground — namely, the planet Venus.

(2) Rommel also found that in some cases reporters had been misled by local law officers and that some law officers favored highly exotic theories to account for mutilations. Law enforcement officers thus appear to play (if unwittingly) as great a role in the mutilations hysteria as they do in the UFO pseudo-mysteries.

Among UFO believers it is an article of faith that law enforcement officers necessarily are competent and reliable observers (of the things they report as UFOs). But experienced investigators find no evidence to support this belief. On the contrary, one investigator discovered that *more* misidentifications of common aerial phenomena are reported by law enforcement officers than by any other occupational group (Allan Hendry, *The UFO Handbook*, Doubleday, 1979, p. 102).

In 1974, CSICOP UFO Subcommittee member Elmer Kral investigated a UFO flap in Nebraska and found that law officers had misidentified the planet Jupiter, and Capella, Sirius, and 30 other stars!

On other occasions law officers in patrol cars have (a) chased aircraft, the moon, and the star Arcturus, all of which they reported as UFOs; (b) reported chasing and being chased by Venus at speeds up to 70 mph; (c) reported stars as triangular UFOs; and (d) reported Venus as a UFO 500 feet away.

These and countless other examples in the UFO literature demonstrate the need for extreme caution when evaluating "official" reports of animal mutilations or UFO observations — especially when law enforcement officers are themselves the observers.

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Fundamental dangers

What is truly irritating (and disturbing) about the article "Superstitions: Old and New" by William Sims Bainbridge and Rodney Stark (*SI*, Summer 1980) is the authors' conclusion. Of course fundamentalists "reject a wide range of occult or pseudoscientific ideas." But they reject these ideas not because they are pseudoscientific but because they are perceived to be diabolical. It is this view of one's opponents as Satanic that makes the fundamentalists so very, very dangerous. To the fundamentalist the secular humanist is even more diabolical than the occultist.

I get the feeling that some of my fellow CSICOP members have become a trifle unbalanced after years of battling pseudoscience and occultism. They have come to believe that the spoon-benders, the astrologers, and the UFOlogists represent a real threat to civilization. Nonsense. They are at worst a minor irritation. There is no great push to have astrology taught alongside astronomy in our schools. Yet our new president came out during the campaign for making creationism the equal of evolution in the school curriculum.

Over the years I have talked to, or corresponded with, hundreds of people involved in what I consider pseudoscience and the occult. Aside from the abysmally stupid, or the completely fraudulent, most of these individuals have tried to arrive at a rational approach to phenomena they did not understand. Sure I think they got it all wrong, but their approach is far better than the one summed up in the fundamentalist bumper sticker: "The Bible said it, I believe it and that settles it."

The fundamentalists have been very effective in suppressing the discussion or teaching of pseudoscientific and occult subjects. At the height of the TM craze, for example, they kept TM courses out of many schools. They have chased "psychic fairs" out of shopping malls and occult books out of libraries and off book-club lists. Perhaps they have even influenced television programming, which is more than the CSICOP has ever been able to do. But for my part I am going to vigorously support the right of any occultist or pseudoscientist to speak anywhere, and say anything, before I will throw in my lot with those who would silence them first, and me next.

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The Committee for the Scientific Investigation of Claims of the Paranormal

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